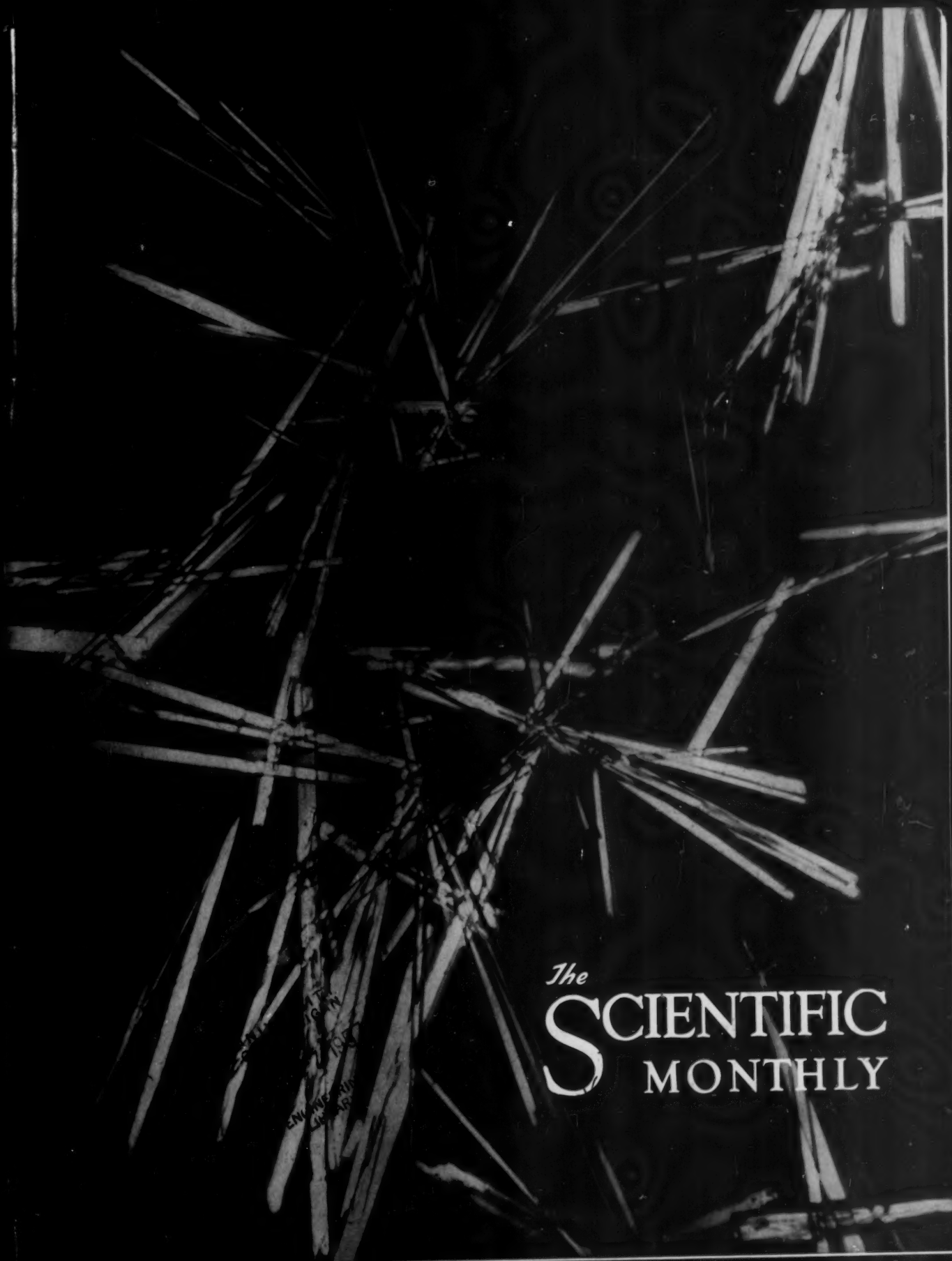

The SCIENTIFIC MONTHLY

VOL. 85 NO. 1 JULY 1957

Giant over-the-horizon antenna designed by Bell Telephone Laboratories for "White Alice," Air Force Alaskan defense communications network.

How UHF radio got seven-league boots

THE huge antenna systems which project ultra-high frequency radio communications beyond the horizon began when a Bell Telephone Laboratories engineer became intrigued with a strange phenomenon. Although these radio waves were supposed to be useful only over line-of-sight distances, the waves displayed a mysterious tendency to take off in a giant stride to antennas beyond the horizon.

This phenomenon had been studied both here and abroad, but no practical use was seen until the engineer became interested and thoroughly sifted the experimental data. He came up with the stimulating conclusion that over-the-horizon transmission is far stronger and much more dependable than was generally supposed. Further he predicted that it could be utilized to supply dependable broadband communications. He and his associates at Bell Laboratories confirmed the prediction experimentally, then drew up requirements for the first over-the-horizon UHF transmission system.

This pioneer work at Bell Telephone Laboratories has greatly increased the usefulness of UHF communications. For example, over-the-horizon transmission now provides critically important communications between remote military outposts in the Arctic and in the far north.

For the Bell System it can provide important new links for telephone and television.

Left, Kenneth Bullington, B.S.E.E., University of New Mexico; M.S., Massachusetts Institute of Technology; recipient of the 1956 Morris Liebmann Memorial Prize and the 1956 Stuart Ballantine Medal for his contributions in the field of over-the-horizon UHF radio transmission.

Right, experimental antenna used in early over-the-horizon UHF radio transmission research. Research extended transmission reach from 30 miles line-of-sight to 200 miles.

BELL TELEPHONE LABORATORIES

WORLD CENTER OF COMMUNICATIONS RESEARCH AND DEVELOPMENT

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[*Courtesy Ciba Pharmaceutical Products, Inc., see page 9*]

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Science and Technology

All inquiries concerning items listed here should be addressed to The Scientific Monthly, *Room 740, 11 W. 42 St., New York 36, N.Y. Include the name(s) of the manufacturer(s) and the department number(s).*

Photomicrographic Light Source

Steady, high-intensity illumination for normal viewing is provided by a photomicrographic light source that can also be pulsed at a very much higher intensity to make a photographic exposure. The increased illumination is obtained by furnishing a pulse of increased power to the xenon-arc lamp. The increase in brightness is approximately 36 times in the type 505 BP unit. The duration of the light pulse can be varied in four discrete steps up to a maximum of 135 msec. The color temperature is very close to daylight. The pulse can be synchronized by camera shutter contacts. (Nems-Clarke, Inc., Dept. M256)

Antenna Horns

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Dewar flasks of stainless steel have a radiant heat transfer of less than 3000 cal/hr at 300°F ambient, with temperature of contents 40°F. Conduction through the neck in a 4-in. length is less than 20 cal/hr deg C. The flasks are of welded construction. (Consolidated Electrodynamics Corp., Dept. M350)

Tube Tester

Electron tube tester uses perforated cards to effect the proper circuit adjustments for tube testing. The testing procedure consists of insertion of the tube, selection of the card appropriate to the tube type, and pushing of buttons to observe quality. (Hickok Electrical Instrument Co., Dept. M245)

Infrared Spectrophotometer

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Here Dr. W. J. Muehlner (left), Dr. H. N. Leifer (center) and Dr. K. F. Cuff discuss properties of solids.

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THE
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JULY 1957

History of Tension

ALDOUS HUXLEY

Mr. Huxley is a distinguished British journalist and novelist. This article is based on an address given at the Conference on Meprobamate and Other Agents Used in Mental Disturbances at the New York Academy of Sciences, 18 Oct. 1956. It is reprinted here by permission from the monograph Meprobamate and Other Agents Used in Mental Disturbances [*Ann. N.Y. Acad. Sci.* **54**, *art. 10 (6 May 1957)*].

THE title of this article is somewhat misleading, for, strictly speaking, the history of tension does not exist. Tension is a form of disease, and diseases, as such, are beyond the scope of history. There is no such thing, for example, as a medieval stomachache, no such thing as a specifically neolithic focal infection, a characteristically Victorian neuralgia, or a New Deal epilepsy. So far as the patient is concerned, the symptoms of his illness are a completely personal experience—an experience to which the public life of nations, the events recorded in the headlines or discussed in scientific journals and literary reviews, are totally irrelevant. Politics, culture, the march of civilization, all the marvels of nature, all the triumphs of art and science and technology—these things exist for the healthy, not for the sick. The sick are aware only of their private pains and miseries, only of what goes on within the four walls of the sickroom. For them, the infinite universe has contracted almost to a point. Nothing remains of it but their own suffering bodies, their own numbed or tormented minds. Disease as an actual experience is more or less completely independent of time and place. Consequently, there cannot be a history of disease as experience; there can be only a history of medicine—that is to say, a history of theories about the nature of diseases and of the recipes employed, at different times, for their treatment, together with a history of the ways in which organized societies have reacted to the problems of disease within the community.

Although tension, as a psychosomatic illness, has no history, at least some of the causes of tension lie within the public domain and can be made the subject of historical study. The same is true of the procedures sanctioned by various societies for the prevention and relief of tension. The subject is enormous, and my space is limited and my ignorance is encyclopedic. I shall therefore make no attempt to discuss all the historical factors associated with tension but shall confine myself to those which are most manageable and, at the same time, most relevant to the problems confronting us today.

Historical Causes of Tension

Let me start with what I shall *not* discuss. I shall not discuss, except perhaps incidentally, the historical causes of tension. This would entail a discussion of two vast and complex themes—the transformation of culture patterns and the relations subsisting between a given culture and the individuals brought up within it. At the risk of indulging in those Original Sins of the intellect, oversimplification and overabstraction, let me sum up this whole matter in one large, comprehensive generalization.

Tension, I would say, arises in persons who, because of some congenital or acquired weakness, are unable to cope with certain distressing situations. These distressing situations are produced by conflict—conflict between the fundamental drives to self-affirmation and sex, on the one hand, and the equally fundamental drive to gregariousness, on the other. The drive to gregariousness is canalized by society, sanctioned by tradition, and rationalized in terms of religion and philosophy; hence, the intrusion of historical factors into a situation which, on the animal level, would be exclusively biological. The disease of tension seems to have arisen under all cultural conditions—in shame cultures as in guilt cultures, in primitive cultures no less than in highly developed cultures—and fundamentally similar devices for the relief of tension have been developed in all the societies of which we have any knowledge. It is with these devices for the relief of tension that I shall be concerned in this article.

His Sweating Self

Like all other diseases, tension tends to narrow the patient's awareness, until, in extreme cases, he is conscious of nothing but himself. Grave illnesses profoundly change the personality of their victims. To this changed personality the narrowing of awareness, induced by the illness, soon comes to seem almost normal, and is taken for granted. Tension is not a severe illness, and those who suffer from tension are well enough to feel and suffer from the cramping self-centeredness imposed upon them by their psychosomatic disorder. They are like those lost souls, whose punishment is, in the words of the great Catholic poet, Gerard Manley Hopkins, "to be their sweating selves, but worse."

The victim of tension knows and is acutely distressed by this sense of being his sweating self, but worse. And here we may remark that even healthy people are often distressed by the realization that they are condemned to be the separate, insulated individuals they so irretrievably are. Neurotics hate being their sweating selves, but worse. Normal people just hate being their sweating selves, period. One of the most disagreeable symptoms of tension is simply the normal distress at being an island universe raised, so to speak, to a higher power. Man is a self-adoring egotist, but an egotist who often feels an intense distaste for the object of his idolatrous worship.

Correlated with this distaste for the beloved self there exists in all human beings an urge to self-transcendence, a wish to escape from the prison of personality, a longing to become something other and greater than the all-too-familiar Me, a susceptibility to a nostalgia for a world superior to, or at least different from, the boring or painful universe of everyday reality. The religious man has attributed this universal urge to self-transcendence to an innate and deep-seated yearning for the divine. The biologist sees the matter somewhat differently and attributes man's desire for self-transcendence to the workings of his innate gregariousness. The individual longs to be merged with the herd, but he is too self-centered to be able to do so completely and too self-conscious to be able to keep up the attempt for very long. He is therefore condemned to live in a state of chronic dissatisfaction, constantly pining for something which, in the very nature of things, he can never have.

These two explanations are not mutually exclusive, and I would be inclined to think that both are partially correct. Be that as it may, the facts which they profess to account for are genuine facts. There *is* an urge to self-transcendence and, along with it, a profound distaste for the insulated ego—a distaste which, in the victims of tension, becomes acute and agonizing. In every human culture certain procedures for achieving temporary self-transcendence, and thereby relieving tension, have been developed and systematically employed. These procedures may be classified under a few comprehensive headings. There are the chemical methods, the musical and gymnastic methods, the methods that depend on subjecting insulated individuals to the influence of crowds, the various religious methods, and, finally, the methods whose purpose is mystical self-transcendence—the various yogas and spiritual exercises of Oriental and Western tradition. Hours would be needed to do justice to all these methods, and I must limit myself to a discussion of only two of them, the most popular and the hardest to control—namely, the chemical method and what may be called the crowd method.

Chemicals for Self-Transcendence

Certain chemical compounds produce certain changes of consciousness and so permit a measure of self-transcendence and a temporary relief of tension. Thus, the so-called "tranquilizing drugs" are merely the latest addition to a long list of chemicals which have been used from time immemorial for changing the quality of consciousness and so making possible some degree of self-transcendence and a temporary release from tension. Let us always remember that, while modern pharmacology has given us a host of new synthetics, it has made no basic discoveries in the field of the natural drugs; it has merely improved the methods of extraction, purification, and combination. All the

naturally occurring sedatives, narcotics, euphorics, hallucinogens, and excitants were discovered thousands of years ago, before the dawn of civilization. This surely is one of the strangest facts in that long catalog of improbabilities known as human history. Primitive man, it is evident, experimented with every root, twig, leaf, and flower, every seed, nut, berry, and fungus in his environment. Pharmacology is older than agriculture. There is good reason to believe that even in paleolithic times, while he was still a hunter and a food-gatherer, man killed his animal and human enemies with poisoned arrows. By the late Stone Age he was systematically poisoning himself. The presence of poppy heads in the kitchen middens of the Swiss Lake Dwellers shows how early in his history man discovered the techniques of self-transcendence through drugs. There were dope addicts long before there were farmers.

Here let me mention a fact of some importance. To relieve tension, a chemical compound does not have to have the characteristics of a tranquilizer. Alcohol, for example, is far from tranquilizing, at least in the middle stages of intoxication. But alcohol has been relieving tension ever since Noah made his epoch-making discovery. Self-transcendence can be achieved by an excitant as well as by a narcotic or a hallucinogen. Tension is relieved, not only by such contemplative drugs as opium, peyote, kava, and ayahuasca, but also by the active, extraverted intoxicants, such as wine, hashish, and the soma of ancient India. Physiologically and socially, some drugs are much less harmful than others and, therefore, are to be preferred. But such merely utilitarian considerations have never carried much weight with the drug taker. For him anything that produces a measure of self-transcendence and release seems good. As long as it works here and now, who cares what may happen later on?

A highly significant statement on this subject was made by William James in his *Varieties of Religious Experience*: "The sway of alcohol over mankind is unquestionably due to its power to stimulate the mystical faculties of human nature, usually crushed to earth by the cold facts and dry criticisms of the sober hour. Sobriety diminishes, discriminates and says *No*; drunkenness expands, unites and says *Yes*. It is in fact the great exciter of the *Yes* function in man. It brings its votary from the chill periphery of things to the radiant core. It makes him for the moment one with truth. Not through mere perversity do men run after it. To the poor and the unlettered it stands in the place of symphony concerts and of literature. It is part of the deeper mystery and tragedy of life that whiffs and gleams of something that we immediately recognize as excellent should be vouchsafed to so many of us only in the fleeting earlier stages of what in its totality is so degrading a poison. The drunken consciousness is one bit of the mystic consciousness, and our total opinion of it must find its place in our opinion of that larger whole."

Elsewhere in the *Varieties* James cites the dictum of one of his medical friends: "There is no cure for dipsomania except religiomania." In their somewhat too epigrammatic way, these words express a truth, which the collective experience of Alcoholics Anonymous has amply confirmed. Mystical experience stands to drunkenness in the relation of whole to part, of health to sickness. For the alcoholic as for the mystic, there is an opening of doors, a bypassing of what I have called the cerebral reducing valve, the normal brain function which limits our mental processes to an awareness, most of the time, of what is biologically useful. For both, there is a glimpse of something transcendent to the world of everyday experience—that narrow, utilitarian world which our self-centered consciousness selects from out of the infinite wealth of cosmic potentialities. What the drunkard sees in the earlier phases of intoxication is immediately recognized as excellent. What is not excellent is the particular method employed for achieving this transcendental experience.

Alcohol is one of the oldest and certainly the most widely used of all consciousness-changing drugs. Unfortunately, it is a rather inefficient and, at the same time, a rather dangerous drug. There are other and better ways than getting drunk for achieving the same intrinsically excellent results. Some of these ways are chemical, others are psychological. Others involve fasting, voluntary insomnia, and various forms of self-torture. All these procedures modify the normal body chemistry and so facilitate the bypassing of the cerebral reducing valve and a temporary escape from the prison of insulated selfhood. Some day, when psychology becomes a genuine science, all these traditional methods for producing self-transcendence will be systematically examined, and their respective merits and defects will be accurately assessed. For the present we must be content with such fragmentary knowledge as is now available.

William James' characterization of alcohol as an exciter of the mystical faculties is strikingly confirmed by what the mystics themselves have said of their ecstatic experiences. In the mystical literature of Islam, metaphors derived from wine and winebibbing are constantly employed. Precisely similar metaphors are to be found in the writings of some of the greatest Christian saints. Thus, St.

John of the Cross calls his soul *la interior bodega di mi Amado*—the inward wine cellar of my Beloved. And St. Teresa of Avila tells us that she "regards the center of our soul as a cellar, into which God admits us when and as it pleases Him, so as to intoxicate us with the delicious wine of His grace."

Consciousness-Changing Chemicals and Religion

The experience of self-transcendence and the release of tension produced by alcohol and the other consciousness-changing chemicals is so wonderful, so blessed, and so blissful that men have found it quite natural to identify the drugs to which they owe their momentary happiness with one or another of their gods. "Religion," said Karl Marx, "is the opium of the people." It would be at least as true to say that opium is the religion of the people. A few mystics have compared the state of ecstasy to drunkenness; but innumerable drinkers, smokers, chewers, and snuff-takers have achieved a form of ecstatic release through the use of drugs. The supernatural qualities of this mental state are projected outward upon the drugs that produced it. Thus, in Greece, wine was not merely sacred to Dionysus; wine *was* Dionysus. Bacchus was called Theoinos—Godwine—a single word that equates alcohol with deity, the experience of drunkenness with the holy spirit. "Born a god," said Euripides, "Bacchus is poured out in libations to the gods, and through him men receive good." That good, according to the Greeks, was of many kinds—physical health, mental illumination, the gift of prophesying, the ecstatic sense of being one with divine truth. Similarly, in ancient India, the juice of the soma plant (whatever that plant may have been) was not merely sacred to Indra, the hero-god of battles; it *was* Indra. And at the same time it was Indra's *alter ego*, a god in its own right.

Many similar examples of this identification of a consciousness-changing drug with some god of the local pantheon could be cited. In Siberia and Central America various species of hallucinogenic mushrooms are regarded as gods. The Indians of the southwestern United States identified the péyote cactus with native deities and, in recent years, with the Holy Ghost of Christian theology. In classical times the northern barbarians who drank malt liquor worshiped their beer under the name of Sabazius. Beer was also a god for the Celtic peoples, as mead seemed divine to the Scandinavians and the Teutons. In Anglo-Saxon, the idea of catastrophe, of panic, of the ultimate in horror and disaster is conveyed by a word whose literal meaning is "the deprivation of mead."

Almost everywhere the consumption of consciousness-changing drugs has been associated, at one time or another, with religious ritual. Drinking, chewing, inhaling, and snuff-taking have been regarded as sacramental acts, sanctioned by tradition and rationalized in terms of the prevailing theology. In the Moslem world, alcohol was forbidden, but the urge to self-transcendence could not be suppressed, and there were and still are places within the Moslem world where the consumption of *Cannabis indica* is not only sanctioned by society but has even been turned into a kind of religious rite. Certain Mohammedan authors have seen in hashish the equivalent of the sacramental bread and wine of the Christians. Among the Jews, many efforts were made to give a religious sanction to winebibbing. Jeremiah refers to the "cup of consolation," which was administered to the bereaved. Amos speaks of men who drink wine in the house of their God. Micah has some harsh words for those who, in his day, used to prophesy under the influence of alcohol. Isaiah denounces the priests and prophets who have "erred through strong drink." They have erred, he says, "in vision." Traditionally, Dionysus was the god of prophecy and inspiration, but, alas, the revelations of alcohol are not altogether reliable.

Herd-Poisoning

From self-transcendence by chemical means, we now pass to self-transcendence by social means. The individual makes direct contact with society in two ways: as a member of some familial, professional, or religious group, or as a member of a crowd. A group is purposive and structured; a crowd is chaotic, serves no particular purpose, and is capable of anything except intelligent action. Using an analogy that is not too misleading, we can say that the first is an organ of the body politic, the second is a kind of tumor, generally benign, but sometimes horribly malignant.

The greater part of most people's lives is passed in groups. Participation in crowd activities is a relatively rare event. This is fortunate, for individuals in a crowd are different from, and in every respect worse than, individuals in isolation or within purposive and organized groups. A man in a crowd loses his personal identity, and this, of course, is why he likes to be in a crowd. Personal identity is what he longs to transcend, to escape from. Unfortunately, the members of a crowd lose more than their personal identity; they also lose their powers of reasoning and their capacity for moral choice. Their suggestibility is increased to

the point where they cease to have any judgment or will of their own. They become very excitable, they lose all sense of individual or collective responsibility, they are subject to sudden and violent accesses of rage, enthusiasm, and panic, and they become capable of performing the most monstrous, the most completely senseless acts of violence, usually against others, but sometimes against themselves. In a word, a man in a crowd behaves as if he had swallowed a large dose of some powerful intoxicant. He is a victim of what may be called herd-poisoning. Like alcohol, herd-poison is an active, extraverted drug. It changes the quality of individual consciousness in the direction of frenzy and makes possible a high degree of downward self-transcendence. The crowd-intoxicated individual escapes from insulated selfhood into a kind of subhuman mindlessness.

From the beginning, men have done their work and gone through the serious business of living in purposeful groups. Crowds have provided them with their psychological vacations. Nourishment drawn from the group has been their staple food; herd-poison has been their delicious dope. Religion has everywhere sanctioned and rationalized intoxication by herd-poison, just as it has sanctioned and rationalized the use of consciousness-changing chemicals. Alfred North Whitehead's statement that "religion is what the individual does with his solitariness" is true only if we choose to define religion as something which, as a matter of historical fact, it has never been, except for a small minority. And the same would be true of a definition of religion in terms of what the individual does with his experience of being in a small, dedicated group, such as the Quaker meeting, or the "two or three gathered together in my name," of whom Christ spoke in the gospel.

The spirituality of small groups is a very high form of religion; but it is not the only or the commonest form—it is merely the best. Significantly enough, Christ promised to be in the midst of a group of two or three. He did not promise to be present in a crowd. Where two or three thousand, or two or three tens of thousands, are gathered together, the indwelling presence is generally of a very different and un-Christlike kind. Yet such crowd activities as the mass revival meeting and the pilgrimage are sanctioned and even actively encouraged by religious leaders today, just as they were in the pagan past. The reason is simple. Most people find it easier to achieve self-transcendence and relief from tension in a crowd than in a small group or when they are by themselves. These herd-poisonings in the name of religion are not particularly beneficial. They merely provide brief holidays from insulated self-consciousness.

The history of man's efforts to find self-transcendence in crowds is long and, for all its strangeness, its weird aberrations, profoundly monotonous. From the potlatch and the corroboree to the latest outburst of "rock 'n roll," the manifestations of herd-poisoning exhibit the same, subhuman characteristics. At their best, such performances are merely grotesque in their subhumanity; at their worst, they are both grotesque and horrible. One thinks, for example, of the festivals of the Syrian goddess, in the course of which, under the maddening influence of herd-poison and priestly suggestion, men castrated themselves and women lacerated their breasts. One thinks of Greek maenadism, with its savage dismemberment of living victims. One thinks of the Roman saturnalia. One thinks of all the outbursts of crowd-intoxication during the Middle Ages—the children's crusades, the periodical orgies of collective flagellation and those strange dancing manias, in which self-transcendence through herd-poisoning was combined with self-transcendence by gymnastic means and self-transcendence through repetitive music. One thinks of the wild religious revivals, the frantic stampedes of those who believed that the end of the world was at hand, the frenzies of iconoclasm in the name of God, of senseless destruction for righteousness' sake. These are bad enough; but there is something much worse—the crowd-intoxication that is exploited by the ambitious rabble-rouser for his own political or religious ends.

In the spring of 1954, while I was staying at Ismailia on the Suez Canal, I was taken by my hosts to the local movie theater. The film, which was drawing record crowds, was *Julius Caesar* played in English, but with Arabic subtitles. The spectators sat in spellbound attention, their eyes riveted on the screen. Why on earth, I kept wondering, should 20th-century Arabs be so passionately interested in a 16th-century Englishman's account of events that had taken place at Rome in the first century B.C.? And suddenly it was obvious. Caesar, Brutus, Antony, all those upper-class politicians fighting for power and in the process cynically flattering and exploiting a proletarian mob which they despised but could not do without, were thoroughly familiar and contemporary figures to the Egyptian audience. What had happened in Rome just before and after Caesar's murder was very like what had been happening only a few weeks before in Cairo, when Naguib fell, rose again in triumph, and was once more brought low by a rival who knew how to play on the passions of the

crowd, how to make use of its drunken enthusiasm and drunken violence for his own purposes. Looking at Shakespeare's play, the movie-goers of Ismailia found themselves looking at an uncensored report on the latest *coup d'état*.

Of course, the greatest virtuoso in the art of exploiting the symptoms of herd-poisoning was Adolf Hitler. The Nazis did their work with scientific thoroughness. All the resources of modern technology were mobilized in order to reduce the greatest possible number of people to the lowest possible state of downward self-transcendence. Phonographs repeated slogans. Loud-speakers poured forth the brassy and strongly accented music, whose repetition drives people out of their minds. Concealed sound machines produced subsonic vibrations at the critical, soul-stirring rate of 14 cycles per second. Modern methods of transportation were used to assemble thousands of the faithful under the floodlights in enormous stadiums, and the voice of the arch-hypnotist was broadcast by radio to millions more.

"Bliss was it in that dawn to be alive." So wrote Wordsworth of his experience of herd-poisoning in the first, joyful months of the French Revolution. In our own time, millions of men and women and millions of enthusiastic boys and girls have had a similar experience. For the herd-poisoned members of the mobs that are used for the making of revolutions and the buttressing of dictatorial power, the dawn even of Nazism, even of Communism, seems blissful. Unfortunately, dawns are succeeded by laborious and often unpleasant days and evenings. In those later hours of revolutionary history, bliss is likely to be conspicuous by its absence. But at the moment of sunrise, nobody ever thinks of what is likely to happen in the afternoon. Like alcoholics or morphine addicts, the victims of herd-poison are interested only in releasing self-transcendence here and now. "After me the deluge," is their motto. And, sure enough, the deluge punctually arrives.

Partial Solutions

From the history of tension let us turn, in conclusion, to the present and the future. It is clear, I think, that the problem of tension will be completely solved only when we have a perfect society —that is to say, never. Meanwhile, it always remains possible to find partial solutions and temporary palliatives. Let us consider a few practical steps that would be fairly easy to take.

First of all, we might incorporate into our present profoundly unsatisfactory and disappointing system of education a few simple courses in the art of controlling the autonomic nervous system and the subconscious mind. As things now stand, we teach children the principles of good health, good morals, and good thinking, but we do not teach them how to act on these principles. We urge them to make good resolutions, but we do nothing whatever to help them to carry these resolutions into practice.

A main source of tension is the consciousness of miserably failing to do what we know we ought to do. If every child were given some training in what Hornell Hart has called autoconditioning, it would do more for general decency and good feeling than all the sermons ever preached. The next step to be taken is prophylactic in character. Human beings pine for self-transcendence, and getting drunk on herd-poison is one of the most effective methods of taking a holiday from insulated selfhood and the burdens of responsibility. As long as they indulge in crowd-intoxication at football games and carnivals, at revival meetings and the rallies of democratically organized political parties, no harm is done. But we must never forget that the spellbinders, the rabble-rousers, the potential Hitlers are always with us. We must never forget that it is very easy for such men to turn an innocent orgy into an instrument of destruction, into a savage, mindless force, directed to the overthrow of liberty. To prevent them from exploiting crowd-intoxication for their own sinister purposes, we must be perpetually on our guard.

Whether a world inhabited by potential Hitlers, on the one hand, and potential herd-poison addicts, on the other, can ever be made completely safe for rationality and decency seems doubtful, but at least we can try to make it a little safer than it is at present. For example, we can give our children lessons in the elements of general semantics. We can tell them about the frightful dangers of intellectual sin, we can make their flesh creep by a recital of the disastrous consequences to societies and to individuals of the rabble-rouser's oversimplification, overgeneralization, and overabstraction. We can remind them to live in present time and to think concretely and realistically, in terms of observable fact. We can unveil the absurd and discreditable secrets of propaganda and illustrate our lectures by examples drawn from the history of politics, religion, and the advertising industry.

Would such training be effective? Perhaps—or perhaps not. Herd-poison is a very powerful intoxicant. Once they get into a crowd, even upright and sensible men are likely to lose their reason and accept all the suggestions, however nonsensical or

however immoral, that may be given them. All we can hope to accomplish is to make it rather harder for the rabble-rouser to do his nefarious work.

The third step that we have to take will, in fact, be taken, whether or not we like it. Once the seeds of a science have been planted, they tend to sprout and develop autonomously, according to the law of their own being, not according to the laws of *our* being. Pharmacology has now entered upon a period of rapid growth, and it seems quite certain that in the next few years scores of new methods for changing the quality of consciousness will be discovered. As far as the individual human being is concerned, these discoveries will be more important, more genuinely revolutionary, than the recent discoveries in the field of nuclear physics and their application to peacetime uses.

If it does not destroy us, nuclear energy will merely give us more of what we have already—inexpensive power, with its corollary of more gadgets, larger irrigation projects, more efficient transportation, and so forth. It will give us these things at a very high price—an increase in the amount of harmful radiation, with its corollaries of harmful mutations and a permanent fouling of man's genetic pool. But the pharmacologists will give us something that most human beings have never had before. If we want joy, peace, and loving kindness, they can give us loving kindness, peace, and joy. If we want beauty, they will transfigure the outside world for us and open the door to visions of unimaginable richness and significance. If our desire is for life everlasting, they will give us the next best thing—eons of blissful experience miraculously telescoped into a single hour. They will bestow these gifts without exacting the terrible price which, in the past, men had to pay for resorting too frequently to such consciousness-changing drugs as heroin or cocaine or even that good old stand-by, alcohol.

Already we have at our disposal hallucinogens and tranquilizers whose physiological price is amazingly low, and there seems to be every reason to believe that the consciousness-changers and tension-relievers of the future will do their work even more efficiently and at even lower cost to the individual. Human beings will be able to achieve effortlessly what in the past could be achieved only with difficulty, by means of self-control and spiritual exercises. Will this be a good thing for individuals and for societies? Or will it be a bad thing? These are questions to which I do not know the answers. Nor, I may add, does anyone else. The outlines of these answers may begin to appear a generation from now. Meanwhile, all that one can predict with any degree of certainty is that many of our traditional notions about ethics and religion and many of our current views about the nature of the mind will have to be reconsidered and reevaluated in the context of the pharmacological revolution. It will be extremely disturbing; but it will also be enormous fun.

Tranquilizer

Our cover this month shows crystals of Serpasil, a form of reserpine, in a photomicrograph, magnified hundreds of times their actual size. Pure reserpine has the empirical formula $C_{33}H_{40}N_2O_9$ and is obtained from *Rauwolfia serpentina,* a pink-blossomed plant that grows about 2 feet high with long, snakelike roots. Serpasil is one of the tranquilizing drugs, which are now widely used in the treatment of mental disturbances and hypertension. It is manufactured by Ciba Pharmaceutical Products, Inc., Summit, New Jersey.

Guiding Migrant Salmon

PAUL E. FIELDS

Dr. Fields is professor of psychology and fisheries at the University of Washington. He received his training at Ohio Wesleyan and Ohio State universities and was a National Research Council fellow in comparative psychology at Stanford University during 1930–32. He was formerly professor of psychology at Maryville College, Ohio Wesleyan University, Wabash College, and the U.S. Air Force School of Aviation Medicine. During World War II he served as personnel consultant in the Army. This article is based, in part, on papers presented before meetings of the American Association for the Advancement of Science at Berkeley, in 1954, and Pasadena, in 1955, and of the American Psychological Association at Chicago, in 1956.

IT has been estimated that the value of all commercial and sports fisheries to the economy of the state of Washington approximates $100 million annually, with the salmon fishery contributing nearly half of this total. The value of the salmon spawned in the Columbia River system each year may exceed $17 million. The future of this natural resource is now threatened by increased competition for the water of these rivers for power and irrigation and for industrial and personal uses. Even flood control may be a detrimental factor under some conditions, for in common with nearly all plans for the control and utilization of water, it involves the construction of dams, and dams block the passage of fish.

Several reasonably successful methods (fish ladders, fish elevators, and tank trucks) have been devised for helping the adult salmon move upstream, past dams (Fig. 1). The construction costs of these fish facilities at eight major Corps of Engineer projects, either completed, under construction, or authorized, in the Columbia River Basin are estimated to be about $130 million. The operation and maintenance costs for these same projects would approximate $1 million annually (*1*). The Corps of Engineers has been concerned with the magnitude of these costs because there have been no definite criteria for the adult fish facility requirements, and no provision has yet been made for the construction of any facilities for the young, downstream migrants. Since all Pacific salmon die soon after spawning, it is essential that their offspring be guided safely past the dams on their journey to the sea, if the race is to survive.

To facilitate the search for possible solutions, a special fish-behavior laboratory at the University of Washington School of Fisheries has been utilized, under provisions of a contract between the U.S. Army Corps of Engineers and the university, for experimental studies of methods for the control of movement of downstream migrant salmon and steelhead trout. An interdisciplinary research team composed of biochemists, biologists, electrical and hydraulic engineers, mathematicians, physiologists, and psychologists is investigating these problems, with the cooperation of various state, national, and international fisheries organizations.

Magnitude of the Problem

It has been estimated that from 10 to 35 percent of the young, downstream migrants may be injured or destroyed at dams on the Elwha, depending on whether the fish go over the spillway or through the turbines. The type of spillway and the head on the turbines seem to be the determining factors in establishing their respective mortality rates. For example, a 180-foot free fall resulted in no mortality at one dam, but a 100-foot contained spillway at another killed 37 percent of the fingerlings. Similar turbines at the two dams were operated under approximately the same settings, but the one with the higher head had 33-percent mortality as opposed to no noticeable mortality for the other (*2*). In another case, at a 250-foot-high dam, a sliding spillway of the conventional type killed an estimated 64 percent, and the turbines, 34 percent of the young sockeye salmon. There has been a decline of 55 percent in the sockeye salmon run on this river since the dam was constructed (*3*). It is evident that any increase in the number of such dams on a river system constitutes a serious menace to the salmon which spawn in it.

There are at least two approaches to the solution of this problem. One, the short-term and probably more expensive method, involves fundamental changes in the engineering design of spillways and turbines at dams which are not yet constructed, as well as the modification of those now in existence, to make them safe. The other involves the discovery of effective methods for guiding the fish to

designated safe areas. As an example of the first approach, the findings from two recent studies suggest that the mortality suffered by small salmon and trout at certain dams might be materially reduced by changing the design of the spillway. A "ski-jump" spill on a 250-foot dam (*4*) reduced the mortality from 64 percent to 17 percent, and a 300-foot free-fall from a helicopter into a rearing pond (*5*) resulted in about 5-percent mortality for salmon and trout less than 6 inches long (Fig. 2). The necessity for these structural changes will be obviated with the construction of large storage dams capable of controlling the seasonal flow in a river system, so that no water is lost over the spillway. Under this latter condition, as today, it would be necessary to control the behavior of downstream migrant salmon so that they could be guided into a relatively small area in the forebay and then transported past the dam in some as yet undetermined manner.

Methods of Guidance

When this research (*6*) was authorized, in 1952, the only method of downstream guidance which had gained general acceptance was some form of mechanical screen, and these screens were not practical for use in large rivers. Prior to the time that large cylindrical rotating screens were first used, it was estimated that a majority of the young salmon of certain rivers died in the irrigated apple orchards and ranches of eastern Washington. Since introduction of the screens, there has been a significant increase in abundance of adult fish returning to these same rivers. Installations which were constructed several years ago to screen a specific volume of water have been found inadequate after the flow was increased, especially where the water is muddy or loaded with white rock that has been so finely pulverized by the glaciers that it is carried in suspension (glacial flour). For example,

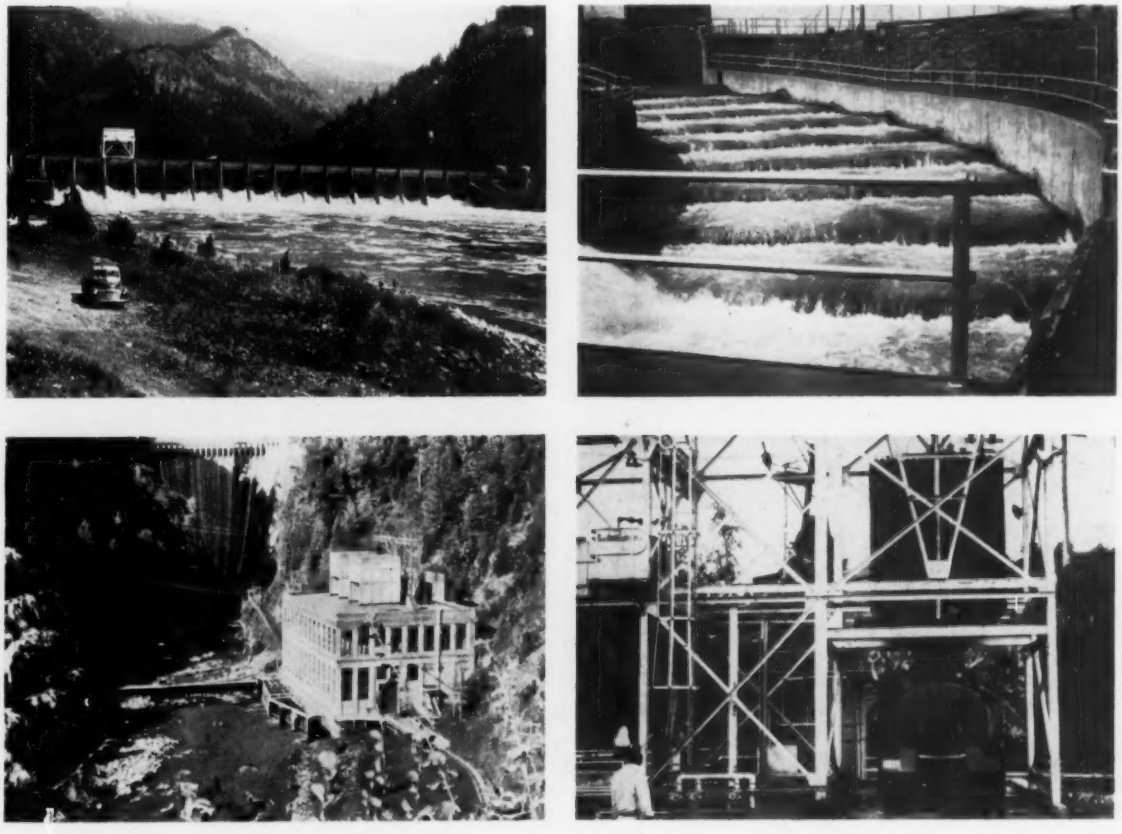

Fig. 1. Assisting adult migrants around dams. (Top) This pool and overfall fishladder is one of the oldest types used to help adult upstream migrants around a low dam such as that on the left. The fish jump over a succession of 12-inch falls until they have climbed to the level of the water above the dam. (Bottom) At high dams the adult migrants may be caught in special traps below the dam and hoisted to the top as in an elevator. At some locations these traps may be emptied into specially aerated tank trucks and the fish then hauled above two dams in a single trip.

from 50 to 75 percent of all chinook and silver salmon less than 1 year old were observed to ride over rotating screens on the White River diversion (*7*). At today's prices, the construction of a battery of similar screens might exceed $250,000.

A number of other methods of guiding downstream migrants have been described in the literature. These include the use of curtains, made by hanging strands of chains or cables in the water, of air bubbles rising from the bottom of the stream, of constant and intermittent lights of various wavelengths, of chemicals (both volatile and nonvolatile) (*8*, pp. 1–65; *9–11*), of air and water vibrations of different frequencies (*12, 13*), of louvers (*14*), and of a wide variety of alternating-current and direct-current electric shocking devices (*3, 8, 15, 16*). None of these have been successfully standardized.

Although there are frequent references to the sensory abilities and learning behavior of fish, very few of these studies are concerned with fish that have any commercial importance, and still fewer, with anadromous salmon. Previous experience has shown that it is not possible to generalize safely from the behavior of other marine fish to salmon. The various species of salmon native to the Columbia River start their downstream journey at different ages and sizes, and the adult upstream migration overlaps portions of the downstream migration. Since no stimulus was known which evoked similar response patterns in all species and sizes of migrants, it became necessary to choose between various sensory-motor responses and select, for extended study, those which seemed to offer the greatest promise of success. If it could be shown that young salmon and steelhead trout possessed certain behavior patterns which were invariably released in the presence of specific stimuli and that these stimuli were not detrimental to adults, then proper control of the conditions under which the stimuli were presented might permit utilization of these unlearned responses in guiding the migrants. The avoiding reaction to light (negative phototaxis) seemed to be a good place to start.

Importance of Vision

It has been observed that, in clear water, downstream migrants travel primarily at night and are found in the top 15 feet of water. They tend to hold position in the daytime by congregating near the bottom or by sinking to the gravel along the banks. They can be readily displaced downstream, in the daytime, by muddying of the water, even though the velocity is not increased. The importance of the visual modality was further demonstrated in the first formal experiment of this series when a total of 568 fish from three age groups and four species of Pacific Coast Salmonidae [silver, chinook, and sockeye salmon and steelhead trout (*Oncorhynchus kisutch, O. tshawytscha, O. nerka,* and *Salmo gairdnerii*)] were tested in a two-choice discrimination apparatus, during the daytime and at night, to determine the response to a chain barrier hung in still water. When the strands of safety chain were not more than 2.5 centimeters apart, 66 percent of the fish were deflected in the daytime and 56 percent at night, while the channel with the slightly higher level of illumination deflected 89 percent. Light was significantly more effective than chains in controlling the behavior of these salmon (*17*).

Because the closely spaced chain barrier had not proved effective in still water, further work with it was abandoned in favor of a thorough investigation of the unexpectedly high efficiency of light as a guiding agent. An attempt was next made to guide young chinook, silver, and sockeye salmon and steelhead and cutthroat trout (*Salmo clarkii*) with various intensities of light without putting any gear in the water. Under these conditions 90 percent of more than 1000 young Salmonidae were successfully deflected into the dark raceway channel by means of an angled barrier of light from a variety of sources. Even in the daytime, a large proportion of the fish would avoid the channel on the bottom of which a white metal plate had been placed, and the percentage of deflection was increased when a 100-watt electric lamp was turned on above it (*18*).

A survey of the literature has revealed no general agreement concerning the response of young salmon to light. For example, Erickson (*19*), McKernan (*20*), and Fry (*21; 22,* pp. 301–332) have reported that lights attract certain sizes and species of salmon. But when the same method of trapping was used in another stream, it was abandoned after a few nights when it became evident that the catch would not exceed 10 to 20 salmon per hour (*22,* p. 306). Lethlean (*16*) and Kerr (*23*) have stated that the behavior of certain species and sizes of salmon was not affected by the lights. Hoar (*24*) observed "cover reactions" and "photosensitivity" in young salmon and assumed that both depth preference and cover reaction might be responses to light. Andrew, Kersey, and Johnson (*8,* p. 8) have obtained inconsistent results with yearling sockeye at Cultus Lake (these results depended on how the lights were used) but, in general, report that the migrants were attracted to the

lights. Their experiments were limited in number because "only on a few occasions did the migration occur during periods of darkness." However, Foerster's earlier report concerning the seaward migration of young sockeye from the same lake stated: "Since the migrants arrived at the counting fence during the night, they were normally counted the following day" (*25*).

Fig. 2. Studying downstream migrant mortality. (Top left) Young salmon using the ski jump on the downstream face of a high dam were shot out into space and fell free into a pool. They had much better survival rates than the migrants that used the sloping concrete spillways. (Top right) Young salmon migrants dropped from a helicopter had a very high survival rate. [Courtesy Washington State Department of Fisheries] (Bottom left) Most irrigation and power canals are now protected by some kind of screening device. Here the cylindrical rotating screens, each 10-feet in diameter, are constantly turned by water-wheel paddles. As they revolve, leaves and other material lodging against the upstream side are carried up and washed off on the downstream side. The young downstream migrants cannot swim through the screen mesh and do not ride over at low water levels. They eventually swim back to the river through large tubes located on each bank of the canal. (Bottom right) Young salmon impinged on the rotating screen in high turbid water are carried on over and lost.

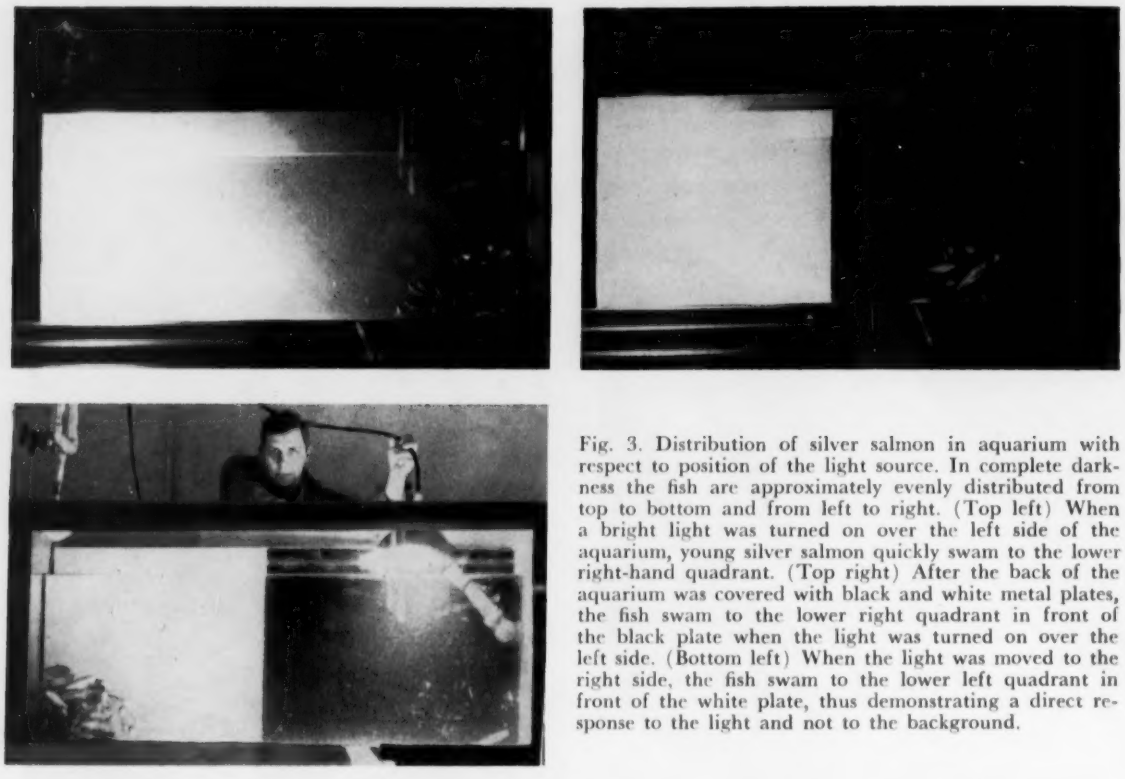

Fig. 3. Distribution of silver salmon in aquarium with respect to position of the light source. In complete darkness the fish are approximately evenly distributed from top to bottom and from left to right. (Top left) When a bright light was turned on over the left side of the aquarium, young silver salmon quickly swam to the lower right-hand quadrant. (Top right) After the back of the aquarium was covered with black and white metal plates, the fish swam to the lower right quadrant in front of the black plate when the light was turned on over the left side. (Bottom left) When the light was moved to the right side, the fish swam to the lower left quadrant in front of the white plate, thus demonstrating a direct response to the light and not to the background.

Prior to the start of the University of Washington research program, a progress report by Brett and MacKinnon (*9*) was the only study which suggested that lights might be used to deflect migrating salmon. Their subsequent paper (*10*) described the use of constant and flashing lights and bubbles in an attempt to deflect young spring chinook salmon and cutthroat and Kamloops trout. They reported that flashing light was very effective in deflecting young chinook but that data for the last two species were not significantly different from chance. In a recent study with sockeye salmon, Brett and Alderdice reported: "Bright artificial lighting at night did not provide better deflection than no lighting" (*26*, p. 18). Warner (*13*) has stated that when sound and air-jets were used at night without any light, no deflection was obtained. The principal obstacle to an evaluation of these studies is the lack of vital information concerning the actual light conditions in effect during the experiments.

In an attempt to resolve some of these conflicting findings, a study (*27*) with more than 1500 fish was undertaken in which the size range was extended from fry that had recently emerged from the gravel (*28*) to fingerlings that were considerably past their migratory period. To check on the possibility of there being species differences in response to light, all five species of Pacific salmon and steelhead trout were investigated. The sounding and lateral displacement of these young hatchery-raised or wild fish in response to the direct rays from electric lamps placed over and adjacent to the two ends of a glass front aquarium were analyzed (Fig. 3). In general, all species of downstream migrants showed a statistically significant preference for the darker portion of the light gradient. There was some evidence that chinook fry reared inside a hatchery had been conditioned to approach the light as a result of the feeding routine. The aggregation in the beam of a flashlight exhibited by some fry and fingerlings was related to the previous level of light adaptation.

Wild pink (*Oncorhynchus gorbuscha*) and chum (*O. keta*) downstream migrant salmon that were brought to the laboratory for testing displayed a significant sounding response but no consistent lateral movement away from the light source. Neave has reported that these fish avoid light and do not emerge from the gravel until dark (*29*). The light-avoiding response was not always present in the early development stages of hatchery silvers and chinooks; at times they showed a definite attraction toward a dim light. When compared with

hatchery-reared fry, wild silver and wild chinook fry displayed a more consistent pattern of light avoidance, although both would gather in a dim flashlight beam after adaptation to a very bright light. Wild steelhead trout and silver, chinook, and sockeye salmon that were trapped at the peak of their individual downstream migrations and brought to the laboratory for testing avoided the brightest part of the light gradient almost perfectly under all test conditions.

The most nearly universal response displayed by all sizes and species of salmon and trout in still water was the avoiding of light. It seemed that the behavior of these fish might be controlled by brightly illuminating the area they were supposed to avoid and darkening the area they were expected to use.

Factorial Studies in Moving Water

With the completion of the Fish Behavior Laboratory at the University of Washington and standardization of an experimental flume (Fig. 4), it became possible to undertake a series of factorially designed experiments in which general illumination, velocity, depth, and temperature of the water, the intensity of lights in the barrier, and the angle at which different light barriers were placed to the water-current were controlled for the first time (*30*). Tests were made of the reactions of 90 different groups of 25 hatchery-reared, yearling silver salmon to a light barrier in every combination of three angles and three brightness levels, in water with three depths and four velocities. Fish which entered the lighted areas were considered guidance failures. The analysis of variance for the different experiments disclosed the following significant results: (i) The smaller the angle of the barrier to the current, the smaller the number of fish that entered the lighted area. (ii) Fewer fish entered the lighted area as the light intensity was increased. (iii) Velocity had the most distinct effect of any of the factors; the number of fish found in the lighted area increased with the velocity of the water. (iv) The difference between 3 and 24 inches of depth did not have any significant effect in the clear water of the flume.

Because of the small size of the experimental flume, the fish might have been able to avoid the light barrier, since they had to move not more than 3 feet laterally while the current was carrying them 12 feet downstream, whereas in a practical field situation they might be called on to maintain this same speed for a much greater distance. It became desirable, therefore, to obtain some measure of the swimming ability of the fish over an extended

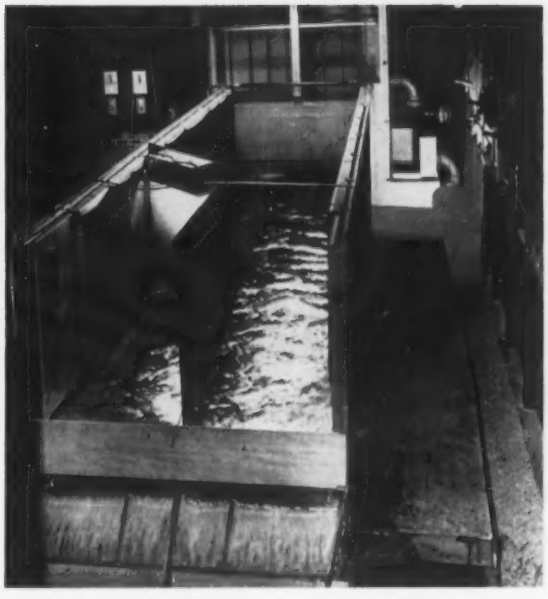

Fig. 4. Experimental flume from downstream end. The 45° light barrier is set to guide from the right to the left side.

period of time. The swimming ability of immature silver salmon was measured in the experimental flume by two methods: (i) measurement of the total time spent in the flume by a fish before it was swept downstream and eventually flattened with its side against the stationary, vertical wire screen and was held there by the force of the water for 30 consecutive seconds (impinged); (ii) measurement of the total time spent in the flume by a fish before it was swept over the falls at the downstream end, across the inclined plane trap, and into the live-box.

The first method probably was the better measure of severe fatigue under strong motivation. An average of 35 percent of the fish were still swimming after 15 hours of continuous exposure to a nominal velocity of 3.4 feet per second (minimum measured velocity of 1.6 feet per second in crests of standing waves). The second method may be as much a measure of motivation as of physical fatigue. All fish were displaced 24 feet downstream in less than 15 minutes, with a nominal velocity of 3.9 feet per second (minimum measured velocity of 3.6 feet per second). When a 20°-angled light barrier with 90 foot-candles of illumination was introduced in one-half of the flume, 80 percent of the fish were able to avoid this side and were impinged on the retaining screen or were displaced over the falls in the dark (*31*).

Not all species of downstream migrants move to the sea at the same time, nor are they all the

same size at the start of the migration. It was important to know whether there were any significant species differences in the manner of responding to a guiding light barrier. Therefore, a total of 72 groups, each containing 50 yearling steelhead trout or chinook or silver salmon with average fork-lengths of 144, 125, and 110 millimeters, respectively, were tested in a factorial study with light barriers, where the angles of the barrier were 90° (blocking) and 20° (guiding) and the levels of illumination were 0.31 and 40.0 foot-candles, in water velocities of 0.37 and 3.90 feet per second. The fish that passed under, or were swimming in, the lighted area of the experimental flume were considered guidance failures.

The data were studied by the analysis-of-variance technique. All primary factors were significant at the .001 probability point. The effectiveness of the light barrier was greatest at the lowest barrier angle, the lowest water velocity, and the highest level of illumination. There was a significant species difference; steelhead were found to be the most sensitive and chinook, the least sensitive to light. An additional 27 groups of all three species were tested in three intermediate velocities, and the number of failures increased as the velocity increased, although not in the same ratio. The silver salmon were always superior to the chinooks, in spite of the latter's greater size (*32*).

One particular weakness is frequently encountered with rotating screens in irrigation and power canals, especially where fish cannot readily find the bypass and where the water level rises above the middle of the screen. Under these conditions the migrants swim ahead of the screens until they no longer have the energy required to stay free of the wire mesh. They are then flattened against the outside cover of the rotating cylinder, and the higher the water extends above the radius of the screen, the easier it is for the fish to be carried over the top and be lost (*7*).

An experiment was designed to investigate the relation between amount of illumination and swimming time before impingement (*33*). A total of 12 groups, each containing 100 yearling-plus steelhead trout or chinook or silver salmon, were tested under four light conditions in an experimental flume with velocities of 3.0 feet per second for 45 minutes and 4.0 feet per second for the next 15 minutes. The time at which each fish was impinged on the ½-inch wire mesh screen at the downstream end of the flume was recorded. The four lighting conditions were: (i) 20°-angled light barrier on one-half of the flume, (ii) 90° blocking light barrier across both sides of the flume, (iii) all light, and (iv) all dark.

Steelhead were found to be least, and chinook most, dependent on the light cues to avoid impingement. The 90° light barrier and over-all light were about equally effective in keeping chinooks and silver salmon off the screen. The 20°-angled barrier was successful in deflecting a significant proportion of all three species into the dark channel after they were fatigued, where they were eventually impinged.

In applying these findings it would seem that lighting the screens and the area immediately upstream from them or, better yet, the use of an angled light barrier leading directly to the bypass should reduce losses over the rotating screens. The results of these studies (*30–33*) point to the necessity of setting the angle of the light barrier to the water flow in accordance with the swimming ability of the weakest or least strongly motivated migrant.

Because significant guidance had not been obtained with the chain barrier in still water, the experiment was repeated in the experimental flume, with flowing water (*34*). A total of 48 groups, each containing 50 yearling steelhead trout or chinook or silver salmon, were tested for guidance with a chain barrier in a factorial study where the strands of chain were spaced 1 and 4 inches apart and the angles of the barrier were 90° (blocking) and 32° (guiding), in water velocities of 0.79 and 1.67 feet per second, with respective depths of 0.60 and 0.43 feet. Significant guidance was not achieved. This confirmed the earlier report of the ineffectiveness of hanging chain barriers (*17*). More steelhead than chinook, and more chinook than silver salmon, were found behind the chain barrier. (These constituted guidance failures.)

Response of Adult Migrants

The young of all five species of Pacific salmon (genus *Oncorhynchus*) and of two species of trout (genus *Salmo*) had demonstrated a significant avoidance response to areas of bright, artificial illumination. Whether adult upstream migrants would show a similar negative phototropic response was also investigated (*35, 36*). A 64-foot, covered approach to the University of Washington fish-ladder was wired so that eight equally spaced 100-watt electric lights underneath the plywood panels could be illuminated on alternate nights (from 5 P.M. to 8 A.M.) during the adult salmon runs. The number of adult sockeye, chinook, and silver salmon ascending the ladder was recorded twice daily.

All species displayed a statistically significant preference for entering during the night over entering during the day and for dark nights over

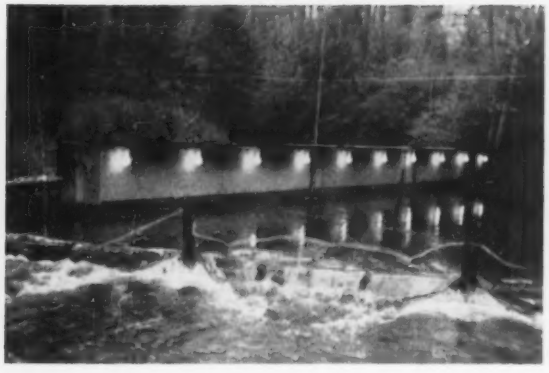

Fig. 5. Light barriers used in Minter Creek validation studies. (Top left) Unshaded light barrier from upstream, guiding to the nonpreferred bank. (Top right) Light barrier from downstream side, lights shaded from approaching fish. Traps shown below dam in most effective position (not used during formal experiment). (Bottom left) Enclosed barrier to give a lighted rectangle 1 foot wide and 40 feet long. Second short barrier shows at the right. In supplementary experiments the enclosed multiple light barriers were shown to be the most effective.

light nights. (There were 41 sockeye returns on dark nights, one on light nights, and none in the daytime.) Sockeye were significantly more sensitive to light than either silver salmon or chinooks; there was no significant difference in response between the latter two species. It was concluded that, under the conditions of these experiments, both young downstream and adult upstream salmon migrants displayed a consistent avoiding response to artificial lights, which might be exploited in guiding them into desired channels.

These results with adult sockeye tend to support the earlier observations made by Ward on the Skagit River (*37*), but do not agree with those reported by Neave for coho and spring salmon on the Cowichan River (*38*). They are also in disagreement with the general picture of adult migration through the fishladder at Bonneville Dam, first presented by Holmes and Morton (*39*) and repeatedly verified since that time. The bases for the different behavior patterns are not known.

Field Validation

The next logical step was the validation of the light barrier in a field-guiding situation with wild, downstream migrants (*40*). An experimental site was prepared on Minter Creek above the influence of the Biological Station's weir pond (Fig. 5). The entire flow was screened, and the fish were caught in four mesh traps. A light barrier, 40 feet long, was pivoted from the center of the stream and was switched on, on alternate nights to guide fish into the number 1 trap, on the right bank, or the number 4 trap, on the left bank. The angle of the barrier was 35° to the line of current. Each side of the barrier was equipped with two sets of ten equally spaced lamps, one of 200 watts and the other of 25 watts. The order in which the various conditions were presented was balanced so that, on any one night, there was one dark, one 200-watt, and one 25-watt, 1-hour period in which the light was shaded from the approaching fish and beamed in a downstream direction, and there was also one dark, one 200-watt, and one 25-watt period in which the lights faced upstream. Each combination of conditions appeared an equal number of times at each hour of the night. Each condition appeared before and after every other condition, two different times.

The formal experiment was run for six nights, thereby permitting duplication of identical light-level sequences at the same hours but with lights shining in opposite directions and with the barrier

angled to the opposite bank. The data were gathered during a 10-day interval in which the downstream migration of silver salmon was increasing.

Analysis of the data has revealed the following statistically significant results. (i) With all other factors partialled out, there was a definite guiding effect between traps, as the result of the lighted barrier. (ii) There was no difference in the total scores for the different light directions (shining downstream or upstream). (iii) The distribution of scores among traps was not related to levels of illumination. (iv) There were differences in the guiding effectiveness of two-term sequences of levels of illumination. (v) The light barrier reduced the number of fish which were trapped as compared with those trapped in an equal number of dark periods. (vi) There was no difference in the effectiveness of guidance with light barriers for hatchery-reared silver salmon, which had been planted in Minter Creek at 3, 6, 9, or 12 months of age, and for the wild silver migrants. (vii) The distribution of wild steelhead and cutthroat trout trapped with the downstream migrant silver salmon also revealed significant guidance by the light barrier.

In a series of one-night exploratory studies, it was shown that the effectiveness of the light barrier could be significantly increased by minor modifications of the installation. (i) There was a gradual reduction in the effectiveness of the light barrier as the period of continuous high illumination (with 200-watt or 25-watt bulbs) was lengthened. This gradual reduction was not apparent when 7½-watt bulbs were used, although they did not completely eliminate the blocking. (ii) The use of an 8-foot light barrier, downstream from the 40-foot barrier, with 7½-watt bulbs, increased the number of fish guided, without any change being made in the position of the positive trap. The use of multiple barriers is suggested. (iii) The guiding effectiveness of a rectangle of light 40 feet long and 1 foot wide directly underneath the barrier seemed to be equal to that obtained when the entire area behind or in front of the barrier was illuminated.

On a number of occasions more migrants were trapped in the first 15 minutes after the lights were turned off in the barrier than had been trapped during the preceding 3 or 4 light hours. This was possible because the fish were able to swim against the current and hold position upstream from the lights. It was assumed that if the velocity were increased to a point where the migrants could not swim against the current, they might still be able to sheer off from the angled light barrier and be successfully guided. With 200-watt lamps burning in the barrier for 4 consecutive hours, it was possible to guide 78 percent of all the fish to the trap farthest to the left of the four (on the preferred side). When the position of the barrier was shifted, with 7½-watt lamps burning for 4 consecutive hours, 71 percent were guided to the right-hand trap on the nonpreferred side.

The installation was later modified in an attempt to guide a release of 16,000 small, hatchery-reared chinooks against their preference with continuous 25-watt downstream illumination of the barrier. In general, the results confirmed those obtained with the silver salmon. Statistically and practically, significant guidance was achieved in moderately clear water with light barriers which did not put any gear beneath the surface (*40*).

Related Problems

Flashing lights. The possible relation of several other factors to successful guidance has been investigated. It has been claimed that a flashing light is more effective than a constant light in guiding downstream migrants (*10*). This question was investigated in the behavior laboratory with 1400 fish (*41*). By means of a 40-foot-candle light barrier set at an angle of 20° to the direction of a 2-feet-per-second flow of water, it was possible to guide statistically significant numbers of yearling silver salmon, released from the right side, across three-fourths of the width of the experimental flume to a channel on the left side. There were seven light conditions, with eight balanced replications of groups of 25 fish per condition in the experimental design.

When the differences were tested by chi-square, it was found that the distribution in the traps in the dark was significantly different from an assumed equal chance distribution. (The migrants were swept out of the flume along the side on which they were introduced.) When the dark distribution was compared with that under the constant light barrier, the difference was highly significant; in the latter case, many migrants swam to the opposite side. The guiding effectiveness of lights flashing at the rate of ⅛ second on and ⅛ second off, ¼ second on and ¼ second off, and ½ second on and ½ second off was not significantly different from the effectiveness of constant light. Lights flashing at ¼ second on and ¾ second off and ⅛ second on and ⅞ second off were significantly less effective than the constant light ($P < .01$ and $< .001$, respectively). When the peak level of illumination was held constant, the average swimming time in the flume was positively related to the "on" time of the lights, with darkness at the lower extreme and constant light at the other.

Adaptation level. It was thought to be quite possible that the effectiveness of a light barrier might be dependent on the level of retinal adaptation at any given moment. An investigation of the effect of three levels of light adaptation upon the guiding effectiveness of an 8-foot-candle light barrier, with 16 groups of 25 young silver salmon, disclosed these results. (i) Control fish, adapted in the dark and tested when the barrier was dark, were all swept out of the flume in the first minute. (ii) Five minutes of light adaptation to 100 foot-candles, followed by a test with the 8-foot-candle barrier, gave slightly better swimming time performance but little guidance. (iii) Adaptation in the dark, followed by a test to the 8-foot-candle barrier, gave the same swimming time as with adaptation to 8 foot-candles followed by a test to a light barrier of the same intensity, but guiding effectiveness was best under the latter condition (*42*).

A more intensive investigation of the role of light adaptation on negative phototaxis was completed with 84 groups of 25 yearling salmon (*43*). In this study, the intensity of the 20°-angled light barrier was held at 10, 20, or 40 foot-candles, and the brightness of the environment in which the fish was adapted was one-half and one-fourth greater, or one-half and one-fourth less, than that of the particular barrier used. There were no significant differences in the guiding effectiveness of the three barrier intensities.

Significant guidance was obtained at each barrier intensity when the brightness of the environment in front was one-fourth or one-half less than the barrier, but the greatest contrast produced the most effective guidance. When the environment was brighter than the barrier, significant numbers of fish swam under the lighted barrier to get into the area of lower intensity. The 21 groups of fish which had experienced one trial in the flume 3 weeks earlier were significantly better than the others. The last of three successive trials showed the greatest light avoidance, in spite of the fact that handling and exercise should have resulted in impaired swimming performance. The most significant improvement came after the first trial. These results confirmed earlier findings that salmon could learn to respond to certain meaningful stimuli after a single experience and could remember it for a considerable period.

Velocity preference. In the course of the Minter Creek field study, it was noted that many of the guidance failures seemed to result from fish which remained close to the banks in areas of low velocity. A laboratory experiment was designed to investigate existence of a possible velocity preference. The reactions of 2300 1-year-plus, hatchery-reared silver salmon were tested in an X-shaped apparatus under conditions of light and dark. As was anticipated, in 13 of the 15 experimental conditions presented, significantly more fish were found in the areas of lower velocity at the end of test periods. Fish did not retain their positions in areas of still water as readily as in areas of some velocity. No preference was shown by these hatchery-reared salmon for upstream or downstream portions of the apparatus when both had closely equivalent velocities (*44*).

Wavelength. An investigation of the speed with which 20 yearling steelhead could be conditioned to red or green filtered light when the brightness was systematically varied revealed significant learning (10 consecutive errorless trials) on the first day, with an average of less than 12 reinforcements (*45*). Infrared light was not effective as a guiding stimulus (*46*). When differential absorption in water is not a factor, colored lights appear to be no more effective for guiding purposes than white lights. Intensity seems to be more important than wavelength.

Electric shock. Because electric shocks have been used as reinforcing stimuli in many learning experiments, several investigations of the primary effects of electricity on guiding behavior have been completed. Certain pulsated direct-current shocks in still water had an adverse effect on the light-avoiding behavior of young silver and sockeye salmon immediately afterward (*47*). In another experiment, all direct-current electric parameters but one were associated with some loss of swimming ability, although only after the heaviest shock were significantly more experimental fish than controls of like size found in the light (guidance failures) (*48*). When extremely weak alternating-current shocks (0.18 volts per inch) were used to condition steelhead trout to a color-reversal problem, frequent repetition of the exposures caused them to "belly up" (*45*). An unsuccessful attempt was also made to find some change in the level of inorganic phosphate and lactic acid in yearling silver salmon which could be related to the number of shocks comparable fish might have received in a field light-conditioning experiment (*49*).

Although young silver salmon that had been subjected to an hour of intermittent electric shocks during a conditioning experiment did not reveal any significant increase in mortality, it was 3 weeks before the vigor of their behavior in a net was judged to equal that of their controls.

The greatest guiding success obtained, to date, with combinations of light and electricity utilized 1.5 cycles of 60-volt, 60-cycle, alternating-current shocks, repeated each ⅔ second across continuous

strip electrodes placed 11 inches apart along the sides of the laboratory flume. Electricity used in this way was a little more effective than light alone when the fish were suddenly released in water moving at greater than 2-feet-per-second velocity. Continuous 60-volt alternating-current shock resulted in immediate paralysis, with no guidance. It is assumed that the strong, interrupted electric shock was a nondirectional stimulus for movement and that light furnished the basis of the orientation. It has been possible to move 83 percent of the silver salmon subjects released against one side across the width of the flume to the trap on the opposite side (*50*). This compares with 58-percent guidance reported by Brett for coho and 89 percent for sockeye migrants where moving white cables and lights were combined in water with a velocity of less than 0.8 feet per second (*26*) and with 99-percent guidance reported by Craddock for sockeye migrants with light alone, in very clear water 44 inches deep, flowing at a velocity of 0.72 to 1.0 feet per second (*51*). These data suggest that repeated exposure to a continuous electric shock is likely to reduce the guiding effectiveness of any light barrier which might be encountered soon thereafter.

Conditioning. Young silver salmon have demonstrated significant avoidance of white light after only one shock, but avoidance of the dark has required more repetitions of the dark-shock situation. The response to a single strong shock (when it was used as the unconditioned stimulus) experienced in the light was not extinguished, in some fish, for more than 60 trials.

More than 100 groups of young silver salmon have been conditioned to avoid an electric shock in still water in a circular or a shuttle-box apparatus by swimming either to the dark or to the lighted area, as the situation demanded. This learned behavior persisted in the original training apparatus, so that highly significant results were obtained in the first 5 seconds of a retest. However, when these same fish were suddenly released (poured from a bucket) in an experimental flume, in water flowing at more than 2 feet per second, there was no significant difference between the behavior of the groups trained to avoid the lights and those trained to approach them. Both avoided the lighted side of the flume. These trained fish were not significantly better than the untrained controls (*50*). It is believed that either the traumatic experience of being "poured" or the stimuli from rapidly flowing water were sufficiently strong to inhibit the previously learned "approach light" behavior patterns and that they were replaced by the innate flight response which led to the dark. This suggests that learning should probably take place in the native stream and that the use of multiple light barriers, combined with some form of punitive stimulation, is indicated.

In another study, each of four experimental groups of 80 young silver salmon was simultaneously trained for 1 hour to swim out of the lighted area to avoid an electric shock. Ten different experimental fish were individually retested against an appropriate control fish, at intervals of 1, 24, 48, 96, and 192 hours. There was no difference in the number of experimental and control fish which swam out of the light in the first 10 seconds after the 96-hour test. Other salmon from these same experimental treatment groups were still noticeably different from their controls several weeks after the original training session if they were tested in small groups instead of individually.

Turbidity. Practically important results were not obtained when an attempt was made to guide migrants in the rapid and turbid water of the White River. The effectiveness of the lights in less rapid water was significant at the start of a related study but deteriorated as the amount of suspended glacial material increased (*52*). It is suggestive that the downstream migration of silver salmon in this stream occurs shortly after the appearance of glacial flour in the lower river and not during the early high water occasioned by melting snow at the lower altitudes.

Conclusions

Many competent individuals have provided us with a variety of personal observations, but the fundamental nature of the response of salmon to light has not been clarified because of the lack of replicated observations. The nature of the light source, wavelength, intensity of illumination (both above water and at the level of the fish's eye), adaptation of the retina as determined by latitude and weather data, diurnal and seasonal cycles, type of light movement (if any), the species, size, and physiological condition of the fish (true migrant as opposed to resident fish which have been displaced downstream by freshets), and water temperature, velocity, turbulence, and turbidity are only a few of the factors which may influence the reaction of the salmon. Perhaps the best way to solve these problems is to put specially trained research teams at three widely separated installations with a single supervisor responsible for standardizing the apparatus and methods employed. Standardization of the experimental light-barrier variables might permit a better evaluation of the other factors involved.

There is a striking similarity in the reactions to

light of downstream migrant silver eels and salmon. Both exhibit a negative phototaxis. Many of the conditions described by Petersen in 1906 for controlling the behavior of the eel work today with salmon. The same misconceptions he encountered concerning the effects of light on the eels are still present with respect to salmon. "It is indeed the general impression everywhere, though not amongst fishermen, that these lights are used to attract the eels, whereas the truth is, that the eels flee from them; but without lights the fishermen would never be able to see the eels at night-time" (*53*).

Each of the experimental studies completed at the University of Washington and summarized here represents a partial step in obtaining control of the movements of downstream migrant salmon and steelhead trout. Light barriers have been emphasized because they have been found to be more consistently effective than any other type of stimulus investigated. Under certain conditions they seem to work for all sizes and varieties of Pacific salmon and trout. They appear to be less expensive to install and operate in large areas than other types of guiding devices, and the light stimulus is not likely to produce detrimental aftereffects related to the size of the fish or the intensity of the light.

The normal response of young salmon and trout to light was found to be negative, with the preference for the darker of two areas strongly ingrained. Whether a light barrier works because the light-avoiding response is a true negative phototaxis, or is an escape reaction based on a fright, or is partially learned is not known with certainty. The strength of the response to a light stimulus, as measured by speed of swimming and resistance to extinction, may be increased as the result of conditioning techniques. The light-avoiding response has been used as an indicator of damage suffered by mechanical and electric means and of general impairment as the result of disease and subsequent medication. For the most effective guidance, the velocity of the water, intensity of barrier illumination, angle of the barrier, and state of retinal adaptation in the fish eye must all be considered. Flashing lights were no more effective than steady ones, colored lights no better than white. Multiple light barriers increased the number of guided fish. Early impingement on a screen was significantly greater in the dark than in the light.

These guiding results have not been validated in a large river under conditions of high velocity and turbidity, nor in the deep impoundments above the high dams. Observations at relatively low velocity indicate that the effectiveness of a light barrier is lost in extremely turbid water. The only stimulus normally encountered in the environment which has seemed to have a greater effect on the behavior of salmon than light is velocity. At the present moment, no method has been found for combining these two stimuli in an effective guiding installation without placing gear under the surface of the water where it would be subject to damage from floods.

When it becomes possible to concentrate large numbers of downstream migrant salmon in small areas, methods of transporting them safely past the turbines and spillways must be provided. Many basic problems remain to be solved, but unless the mortality at dams where significant mortalities occur is drastically reduced, the present rapid rate of increase in the number of such hazards will inevitable destroy this important natural resource.

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On the Rejection of the Martian Canal Hypothesis

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DURING several successive oppositions of Mars after 1877, when G. V. Schiaparelli (*1*) announced discovery of the Martian canals, astronomers all over the world strove to bring out evidence that would either confirm or reject the canal theory. It seemed self-evident then (as it does to most people today) that any report of the existence of rectilinear lines suggesting intelligent construction on Mars must be error caused by illusion or overwrought imagination. But negative proof is the most difficult kind to sustain, and immediate rejection of Schiaparelli's discovery was not accomplished. Owing to the fact that, in a few years, several other astronomers substantiated Schiaparelli's theory, the fashion soon arose of seeing Martian canals in every kind of telescope, big or little.

Among the important confirmations during the oppositions of the period 1892–94 were those of W. W. Campbell (*2*), J. M. Schaeberle (*3*), W. J. Hussey, and E. S. Holden (*4*), for these Lick observers were in command of the new 36-inch refractor, then the world's largest. At Lowell Observatory (Flagstaff, Arizona), Percival Lowell, W. H. Pickering, A. E. Douglass, and others made observations with a superb 24-inch refractor that had been constructed especially for planetary research (*5*). Their work resulted in the publication, in 1895, of reports containing charts that showed hundreds of straight, narrow canals, far more than had been seen at any other observatory.

Canal Theory Confuted

Despite these seemingly firm confirmations, a reaction against belief in the existence of canals on Mars grew among conservative thinkers and fed on their dislike of the speculations that one or two "canalists" had indulged in (*6*). Leaders in the reaction were the American George Hale and the Frenchman E. M. Antoniadi. Hale spoke in 1909, shortly after the 60-inch reflecting telescope had been put into service on Mount Wilson, in California. Antoniadi (*7*), in Paris, at the Meudon Observatory, engaged in correspondence with Hale and received from him a report signed by nine Mount Wilson astronomers, who testified that none of them had been able to see canals with the new telescope. Antoniadi made full use of this devastating report in various ways and, in 1913, he wrote of Martian canals with what may be described as an excess of feeling: "But the astronomer of the future will sneer at these wonders; and the canal fallacy, after retarding progress for a third of a century, is doomed to be relegated to the myths of the past" (*8*).

The principal arguments against the canals are (i) that the eye tends to connect disconnected dots and shaded areas and thus to present these to the mind as continuous lines and (ii) that, though rectilinear markings may often appear in the image of Mars as it is seen in small telescopes of 12 inches and occasionally in medium-sized ones, such as Lowell's 24-inch, nevertheless, when seeing is perfect, the rectilinear markings are resolved by larger telescopes (such as the great Meudon refractor of 32 inches) and take on the appearance of minute and complex details of an entirely natural kind (*9*).

Percival Lowell and Simon Newcomb had debated these points in 1907, and Newcomb had apparently conceded that Lowell's sightings could be accounted for by theoretical considerations (*10*). But scant attention was paid to the outcome of this debate.

Antoniadi's negative opinion, when fortified by Hale and his colleagues, bore much greater weight than Lowell's arguments, because belief was widespread at the time that the larger the telescope, the more authoritative were the announcements of those who peered through the eyepiece. But more recent experience has failed to establish the validity of this belief. Even though, at a much later date, Edison Pettit identified a number of canals in the 60-inch reflector (*11*), this instrument, and those that are even larger, are not considered to be as effective for making Martian observations as are the refracting telescopes that have apertures of 36 inches and less. It has been reported to me that, among members of the Palomar staff, there is a strong feeling that the 200-inch mirror is not an ideal instrument for planetary observation: the reflector was not constructed with that in mind but was designed to extend our knowledge of extragalactic configurations. Furthermore, Albert Wilson has reported that his experiments indicate that a 9-inch refractor at Lowell Observatory is the optimum size when newly developed electronic photographic techniques are used (*12*).

The Aging Observer

But the perspective that we have today has been achieved since the prevailing opinion, which rejects the canals, was formed. Perhaps in 1918 the certitude of Hale's and Antoniadi's disbelief, coupled with negative statements by his own coworker E. E. Barnard (*13*), swung W. W. Campbell, the director of Lick Observatory, over to a negative point of view; or perhaps Campbell in middle age, like Schiaparelli after 50 (*9*), could no longer focus the canal images on the retina of his eye. Whatever the cause, Campbell published a very full critique (*14*) in which he set forth what he regarded as fundamental inconsistencies in the work of Lowell (then deceased), when compared with the canal drawings of the second principal canal exponent, Pickering. Campbell, in this critique, made no reference to the canals that he himself had reported during the years 1892–94 (*2*); instead, he now suggested that the Martian details had been too difficult for human eyes, aided only by existing telescopes, to detect. In 1930, Antoniadi gave the *coup de grâce* to the canal theory by devoting an entire chapter of his new book to "The illusion of the canals"(*9*).

Physical Determinations

After 1909, the attention of most planetary observers was diverted from the topographic study, and their work soon centered around physical determinations of atmospheric movements, vapor pressures, and surface temperatures. The chemical composition of the polar caps and of the atmosphere was also investigated. As a result, it is now well established that, though there is very little water vapor in the Martian atmosphere, that small amount is sufficient to cover the polar regions each winter with precipitated water crystals.

This fact, plus observations of movements of clouds, proves that the atmosphere and its circulation are extensive. Yellow clouds indicate the presence of dust that may arise from wind storms over the Martian deserts or from volcanic activity. According to D. B. McLaughlin, some of the "canals" are traces of dust deposited to leeward of active volcanoes (*15*). The word *canals* has come to be accepted as a conventionalized term to designate certain regions or areas that are more or less rectilinear or bandlike, according to the individual astronomer's opinion; the term no longer signifies "water channel," just as *maria,* when applied to the Moon or to Mars, no longer means "sea," for great bodies of liquid water are unknown on either world. However, some astronomers are still of the opinion that the dark circle that surrounds the melting Martian polar cap is an annular lake.

The Martian climate has been shown, by the temperature measurements of Menzel *et al.,* to be of what might be called an extreme continental type, with a freeze every night and thawing temperatures during summer days (*16*). Although hardly a trace of oxygen is to be found in the planet's atmosphere and higher types of plant and animal life, as we know such life forms, could not exist unprotected on Mars, and although no chlorophyll can be detected by absorption bands in spectrographic studies, nevertheless most astronomers nowadays are willing to consider the possibility that the dark regions—areas that change color with the seasons—are clothed with vegetation (*17*). This vegetation, if it exists, must be of what we call a low order of plant; perhaps it is a type similar to our lichens, which manufacture their own oxygen and which can live under conditions of extreme drouth and cold. The Martian variety would use some molecule other than chlorophyll to catalyze photosynthesis.

Useful as these investigations are in furnishing us with background and understanding of Mars as a world, they do not explain the Martian canals that are persistently reported by a section of the astronomical profession even to this day. If we accept R. J. Trumpler's canal network, neither meteorology, nor volcanism, nor botany, alone, can explain why more than 95 percent of the lines that he draws begin and end at points (oases) (*18*).

Let us review, very briefly, the investigations of that small group of astronomers who have continued to report the presence of canals on Mars after the fall of the canal theory from favor.

Canal Sightings

Emphasis of the canalist school, after 1909, was placed on photography; but even the best photographic plate has less resolving power than the eye. Workers at the observatories of Lick (*19*), Lowell (*20*), Pic du Midi (*21*), and Mount Wilson (*11, 22*) have, as result of these photographic studies, achieved what they regard as objectivity by combining photography with visual observations. V. M. Slipher, of Lowell Observatory, decided in 1927 that the combination method gave complete proof of the existence of the canal network (*20*). In the same period, Trumpler (*19*), of Lick Observatory, took hundreds of photographs of Mars that show canals, and by combining selected negatives, he reinforced the canal images and correlated them with his visual observations. As a result of this method, Trumpler drew maps of Mars that show the existence of a complex and extensive canal network, even as Lowell (*23*) and Schiaparelli had described it (*1*). These confirmations were followed, in 1941, by that of B. F. Lyot, who may be regarded as the French successor of Antoniadi in Martian observations. Lyot reported finding many traces of lines on his photographs of Mars in locations, on the disk, where his visual observations had noted the presence of the darkest canals (*21*). In 1947, Pettit reported confirmation of the canals in a small telescope at Pasadena, California, and also in the 60-inch reflector on Mount Wilson (*11*). Later (in 1953) Pettit reported sighting the pattern of the Lowell network in a 20-inch reflector, and in 1955 he reported that images of a number of these canals appear on photographs that he took in 1954, and that he has become convinced that the network exists, at least to the extent that Schiaparelli described it (*22*).

It is notable that Trumpler, who used the 36-inch Lick refractor in the oppositions of 1924–26 (*19*), agreed with the observers who had reported seeing canals in the same instrument in 1892–94 (*2–4*). Campbell's change of mind (*14*) then and his denial of the canal theory in 1918 were more in accord with the findings at other institutions (*7, 9*) than with what was to be seen by young eyes through the instrument at his own, and this lack of support of Trumpler's findings by the director is probably the reason why Trumpler's confirmation of the network has been so universally disregarded.

Reconsideration of Canal Theory

Trumpler's report and other apparently excellent reports on canal sightings that were made after Antoniadi's denouncements have been publicly ignored and privately derided. In particular, the works of Slipher (*20*) and Jarry-Desloges (*24*) have been so abused. Pettit and Lyot, latter-day canal sighters, have been so cautious in their announcements that few scientists realize that these men have been keeping the Schiaparelli-Lowell point of view alive today, principally in their private circles.

It is my purpose here to inquire whether this turn of events was justified. Was rejection of the Martian canal hypothesis, in accordance with the 1909 statements of Hale and his coworkers (emphasized by Antoniadi), a correct scientific procedure? And if it was not, should the hypothesis be taken up now for reexamination, in view of more recent claims of objective canal sightings?

It would seem that G. P. Kuiper favors such reconsideration, for, although in a recent paper (*25*) he disregards Trumpler and later works and repeats Campbell's argument that astronomers may have been straining to make out features that are beyond them, he does, nevertheless, point out the fact that each generation must satisfy itself about the truth or falsity of the canal theory. Kuiper disqualifies observers on the ground of bias if they have shown a "passionate" interest in the observations. Although the history of science, from Aristotle, through Newton, to Einstein and Oppenheimer, is filled with men who have demonstrated such strong feeling and attachment for their work, and although this emotional drive of genius has not, to my knowledge, been proved to invalidate scientific results, let us, nevertheless, accept, for the present, Kuiper's dictum as applicable to both Lowell and Antoniadi, for these two partisans of opposite poles were "passionately" devoted to their studies. Under this rule, it will be necessary to judge the correctness of the one or the other debater in the light of the sum of confirmations by still other astronomers in order to adopt, at a later date, either of the two debated positions.

Successors have now long been established at both Mount Wilson Observatory and at Meudon Observatory (where the negative view was confirmed) who do not agree with the old evidence on which the rejection of the canal pattern was based. Gerard de Vaucouleurs, a French spokesman of recent years who has endeavored to present the facts impartially, has taken an increasingly more favorable attitude toward the canal theory (*26*).

In view of these facts, I suggest that the frater-

nity of astronomers consider the thought that the gathering of evidence for the canals may be a task for which certain people and observatories are especially adapted (perhaps by unusually keen eyes and by favorable sky and telescope conditions) and that it is necessary to judge the question of the canal network by the record rather than by one's personal ability or inability to see the canals. Such a spirit of open-mindedness is the more timely in view of the trend of current thinking on the possibility of space travel and of rocket technology's promise for the future (*27*, *28*).

Martian Network Not Unique

Throughout the years, the element that has sustained interest in the network of Martian canals is the presumption of their uniqueness among astronomical bodies. While the Martian lines are unique in the sense that like markings cannot be seen on any other object viewed through a telescope, nevertheless, if we were to examine the earth from a distance by means of a telescope, we would discover a topologically identical pattern that arises from man's engineering activity. I have outlined, in the following paragraphs, my communication-network theory, which proves statistically that civilized continents of the earth are covered with lines that are similar in their disposition to the canals of Mars, as defined by Lowell and Trumpler. The following explanation gives the gist of the theory; those who wish to study the full development are referred to other publications (*18*, *29*).

Method of Network Analysis

A surface network is composed of a primary set of points distributed over the surface, with at least three radiant lines connecting every point with three other points of the set. Out of any point set a great variety of networks may be constructed, depending on the manner in which the individual points are connected. A convenient statistical method for classifying networks according to type consists of counting and calculating the percentage of lines in the pattern that radiate from junctions having, respectively, three, four, five, or more connecting rays. It is found that network patterns of nature and artifice, when thus analyzed, fall into different groups. I have selected 158 points which Trumpler, in his 1927 report, calls oases; these constitute junctions from which three or more canals radiate to other junctions of the same group. Approximately 15 percent of the canals in the Martian network of Trumpler (*19*) pass out of junctions of three canals; 43 percent radiate from junctions of four canals, 20 percent from junctions of five canals, 12 percent from junctions of six canals, and 6 percent from junctions of seven canals; and there is one junction in the network with eight canals. In the light of this analysis, we can compare other networks of nature and artifice.

Natural and Artificial Networks

The shrinkage cracks of glazed chinaware, mud cracks in drying ground, and cracks in ancient lava slabs are alike in that by far the greater number of centers have only three cracks radiating outward. I sought, but was unable to find, an example in inanimate nature of a pattern where a preponderance of the rays issue from junctions of four or more. But when intelligent action is a causative factor, such a pattern is found to develop as the result of that intelligence. Spider webs, of course, are an example of many rays issuing from one center; but the spider is unable to intermesh many such loci in the manner of the Martian network. Hence, I turned to works of a higher intelligence and analyzed railroad networks. I found that the railroads of Iowa and Ohio make macroscopic patterns that are topologically identical to Trumpler's and Lowell's Martian networks. Although the Martian network, as thus analyzed, appears to be clearly of the communication type, I do not wish to imply that it has been proved to have a design that would require intelligence equal to man's for its creation. Additional evidence giving more certain criteria for patterns that indicate high intelligence is required before such a theory can be sustained.

Figures 1 and 2 show the air communication network of the earth and the Martian canal network presented in the same style. If our oceans were dry, our railroads would be constructed to cover the surface of the earth after the manner of the Martian network, and in clear weather the lines would be visible from above.

Discovery of New Knowledge

During the summer of 1956, Mars again swung into the point of closest approach to the earth, and planetary astronomers (who are few in number) spent long hours at their telescopes, searching the surface of our nearest neighbor. Unfortunately, the Martian summer solstice occurred so close to the time of opposition that the seasonal canals were not visible in large numbers when Mars presented its largest disk. Robert S. Richardson reports having seen, in the 60-inch reflector on Mount Wilson, a multitude of blue canals on the desert regions of

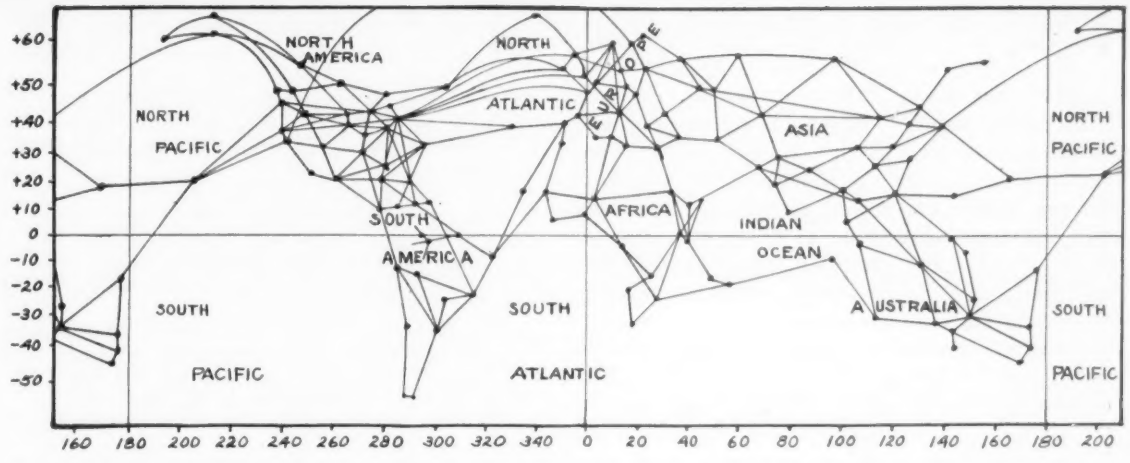

Fig. 1. Air communication network on the earth. [© 1956 by Wells Alan Webb; reprinted from *Astounding Science Fiction*]

the Southern Hemisphere on the morning of 3 June but states that these had disappeared by opposition time in September (*30*). Richardson photographed Mars on 3 June, but the images were too small to show the lines that had been clearly visible to the eye observing Mars directly under high magnification. Perhaps other astronomers have made similar observations and will report them in the course of time. Let us hope that the newer electronic techniques, such as the Lumicon, have yielded decisive results. However, until and unless such techniques are successful, we must rely on the eye of the observer for clearest resolution of Martian details.

I do not pretend to qualify as an observer. Nevertheless, this experience and the following suggestion of observing method seem significant. As a visitor to the Lowell Observatory one night in 1954, I saw several thin black lines stretch across the image of Mars as it appeared in the 24-inch refractor. But these lines would not be steady; they writhed about with rapid movements; they vanished many times and as often reappeared. Clyde Tombaugh told me that these were canals, but the seeing that night was unsteady; it ranged between 3 and 5. My middle-aged eye tired quickly under the strain of trying to make out, and draw, what was appearing in the eyepiece. Clearly, the work of focusing on the eyepiece and of comprehending what is seen sufficiently well to reduce the image to a drawing is a psychological-neurological process that, if considered objectively, has little or nothing to do with the rigorous training in astrophysics and

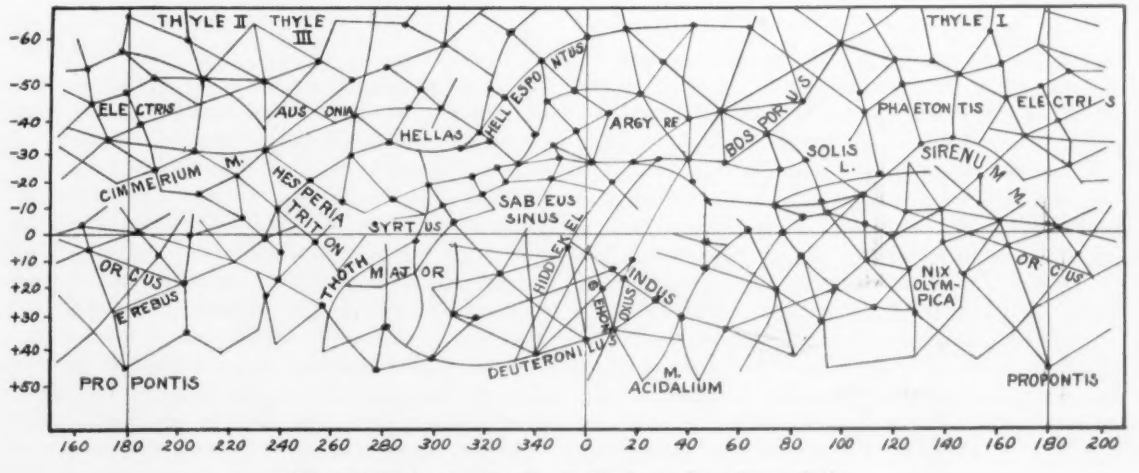

Fig. 2. Martian canal network. [Redrawn from Trumpler]

mathematics that qualifies a man to be an astronomer. Just as the middle-aged coach no longer aspires to the fine coordination of eye and brain that he trains young athletes to exercise in playing basketball or tennis, so the middle-aged astronomer should realize that what may now be, for him, marginal or impossible in Martian observation may lie well within the powers of men in their early 20's who are selected by psychological tests for their quickness of perception, accuracy of vision, and ability to draw, faithfully, the image that appears unsteadily in an eyepiece.

The Air Force has successfully devised such tests and applied them in selecting pilots. Here is an opportunity to use similar methods and tests for the selection of highly competent men to observe Mars—men who would be unfamiliar with past drawings and who would have the duty of making objective reports of appearances for astronomers to interpret. Such men, gifted with keenest sight, adept at drawing, having the quickness of apprehension that only youth can possess, and having been selected, among other things, because of their total disinterest in the outcome, should be able, when placed at suitable telescopes, to provide the drawings that will confirm or disprove the canal hypothesis.

In the tradition of science, hunch and accident have often been the spurs leading to further research and the discovery of new knowledge. With respect to Mars, we now have a statistical correlation (*18, 29*) to back up Lowell's intuitive impression of intelligent design of the canals (*23*). Further research with methods at the command of our generation can solve this question. I can imagine no problem in science that offers promise of greater discovery.

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Mathematics, Abstract Entities, and Modern Semantics

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A SCIENCE can be in a highly advanced state, even though its logical foundations are far from being clarified. Mechanics, for example, reached a stage of astounding perfection by the end of the 18th century, mainly through the genius of Galileo and Newton, although much room was left for controversy about the meanings of its fundamental concepts, length, simultaneity, mass, and force. Even the meaning of the simple law of inertia remained controversial right up to Einstein's "unification" of inertia and gravitation. Similarly, mathematics was not prevented from reaching breath-taking heights of perfection by the *Grundlagenstreit* (dispute about foundations) which began in the 19th century and still continues.

The fundamental question of the philosophy of mathematics concerns the very nature of its subject matter. Traditionally, mathematics was defined as the science of quantity, but this definition was decisively criticized by Bertrand Russell in terms of a conception which reduces pure, abstract mathematics to pure, abstract logic, thereby lifting any restriction to a special subject matter. But though pure mathematics in this conception is, in contrast to the special empirical sciences, unrestricted in subject matter, its propositions are analyzed as referring to classes and attributes. In probing into the foundations of mathematics, therefore, one cannot avoid facing, sooner or later, the old metaphysical problem of the status of abstract entities. The latter has been tied up with the problems of linguistic meaning and reference ever since Plato but never before as closely as at the present time. Modern semantics, as cultivated by analytic philosophers in the United States and England, grew up in close contact with symbolic logic. Accordingly, the problem to be discussed in this article should be of equal concern to mathematicians who reflect on the foundations of their science, to semanticists, and to symbolic logicians.

Traditional Problem of Universals

When a high-school teacher demonstrates that the sum of the interior angles of a triangle equals 180°, he usually draws a triangle on the blackboard, then draws a straight line through the top vertex parallel to the base, in order to be able to reduce the proof of the Euclidean theorem to the proposition that "corresponding" angles are equal. How would he satisfy a "philosophic" pupil who protested that he had not established that all triangles whatsoever have the property in question but only that the particular triangle on the blackboard had it? Obviously, the proper reply would be this: We can be perfectly sure that every Euclidean triangle has the property, without illustrating the proof over and over again by different triangles, because any properties of the particular triangle that distinguish it from other triangles—such as its being equilateral, or its comparatively small size, or its being bounded by white lines—were disregarded in the proof. In the language of Plato: What the mathematician is thinking about when he discovers the geometric proof is not a particular triangle but *triangularity as such*—in other words, that which all particular triangles have in common and by virtue of which they are triangles.

It is this sort of thing that various philosophers, past and present, under Plato's influence call a "universal," or an "intelligible form," or an "essence." These entities were supposed by both Plato and Aristotle to be the objects of scientific thought, as contrasted with "particulars" that are given in sense-experience with a unique location in space and time. Of course, these universals did not have to be geometric forms. In the case of every predicate whatsoever, the things to which the predicate is applicable can be distinguished from the property the possession of which by a particular thing is the criterion of the predicate's applicability: redness

is to be distinguished from red things, hardness from hard things, humanity from particular human beings, and so on.

Again, numbers seemed to be a kind of universals, for, to ask a question which unphilosophic people never ask at all: What is the number 2? It surely is not the symbol "2," for we can use different symbols to talk about the same number (for example, "II," "two," "zwei"). It must, then, be something that the symbol stands for. But it cannot be identified with a particular pair of objects, say a pair of gloves, or a pair of apples, or a pair consisting of wife and husband. It seems, rather, to be something that all particular pairs have in common ("twoness"), an object of abstract thought, not a sensible object.

Once universals are conceived as objects of thought, the realm of universals will be found to be populated not only with those that are somehow "exemplified" in the particulars we sense but also with unexemplified universals: no physical sphere corresponds exactly to the mathematical concept of a sphere; hence, the universal *sphericity* which the mathematician thinks about is not, strictly speaking, exemplified in the physical world. And even if one never physically constructed a regular polygon with 1000 sides, this mathematical object could be reasoned about, and in order that it may be reasoned about it must in some sense "exist," say the Platonists. In the heaven of Platonic forms, then, we find not only perfect mathematical objects but also unexemplified forms that move the imagination of less intellectual people: mermaidhood, centaurhood, unicornhood, and so on. The Platonists' argument seems to be that, in order that something may be thought of, it must in some sense *be*; we can obviously think of mermaids, though there are no physical mermaids in space and time, but we could not have such thoughts unless there existed the universal *mermaidhood*. Since these universals do not exist in space and time, some Platonists try to avoid confusion by calling the sort of "being" that is to be ascribed to them "subsistence" (*1*).

It is important to understand that universals as traditionally conceived, especially by Platonists, are not mental in the sense in which a thought or an act of imagination or a feeling is mental. They are, rather, postulated *objects* of thought whose subsistence is alleged to be independent of their being mentally apprehended in any way. It is for this reason that Platonism is usually called a kind of "realism." Universals are held to be real in the sense that there would be such entities even if no consciousness existed, just as a *physical* realist holds that physical objects are there, whether or not any conscious organism or disembodied mind is aware of them.

The most important traditional arguments, then, for there being universals over and above the world of sensible particulars in space and time are the following two. (i) With the exception of proper names, all meaningful words are *general*. To say that they are meaningful is to say that they stand for something. But, unlike proper names, words like *horse, round,* and *red* do not stand for particular things. Therefore they stand for universals. And these universals would be "there" somehow, even if they were never referred to by means of words. (ii) Every thought has an object; to think is essentially to think *of* something, and the object of one's thought is clearly distinct from the thinking of it. When two mathematicians, for example, converse with each other about the properties of a certain number, they assume without hesitation that they are thinking about the same object and that they are merely trying to discover the properties of that object, in the same sense that a chemist is trying to discover the properties of a compound he is experimenting with, though by essentially different methods. The square root of two would have been irrational, even if no mind had ever thought about it, let alone discovered its irrationality. This second argument is much more tempting than the first. As we shall see presently, the first argument is rather easily disposed of by exposing the underlying confusion between meaningful words and names. But the second argument is a natural outgrowth of a psychological situation with which even nonmathematicians are familiar. Imagine a child who has just learned the meaning of "square number" by means of the examples $4 = 2 \times 2$, $9 = 3 \times 3$, and curiously investigates how many square numbers there are between 1 and 100 and whether their frequency increases or decreases as the numbers increase. He seems to explore a domain of objects that have invariable properties in no way dependent on human thoughts, and yet these objects are not physical objects with spatial location. The child may at first suppose himself to be talking about marks on paper when he says "9 is a square number but 10 is not," but you can easily convince him that these are merely symbols we use for referring to invisible numbers: the sentences "9 is a square number" and "nine is a square number" refer to the same number, but the symbols "9" and "nine" are obviously different.

Modern Semantics and the Traditional Dispute

Many traditional philosophers, especially Occam and his followers in medieval philosophy and the

British empiricists since Hobbes, have tried to discredit Platonism as a belief that has no better rational foundation than a belief in ghosts and fairies. Whatever exists, said the nominalists, is a particular in space and time, and there is no need to postulate any "subsistent" abstract entities. A general word does not stand for a universal but is just an economic device for referring to several particular things that are in certain respects similar. According to the realists, a meaningful general word, like *man*, stands for a universal, and when we judge that a particular object is correctly called "man," we compare it, as it were, to our idea of the universal. But the nominalists denied that we can form ideas of universals—"abstract ideas"—at all. To quote the subtlest of them, George Berkeley (*2*): ". . . it is thought that every name has, or ought to have, one only precise and settled signification, which inclines men to think there are certain abstract, determinate ideas that constitute the true and only immediate signification of each general name; and that it is by the mediation of these abstract ideas that a general name comes to signify any particular thing. Whereas, in truth, there is no such thing as one precise and definite signification annexed to any general name, they all signifying indifferently a great number of particular ideas."

This is a striking anticipation of modern semantic analysis. Berkeley is criticizing the scholastic belief, ultimately derived from Plato, that the generality of a word consists in its representing a determinate entity which is not a particular and of which we have an "abstract" idea. Rather, he held, the generality of a word is its capacity to evoke any one of a set of similar particular ideas. If I apply the general word *triangle* to a particular figure, I do not do so as a result of finding the particular figure to correspond to an abstract idea of triangularity—there is no such thing—but simply because I recognize it as similar in a certain respect to objects with which I have been conditioned to associate the word *triangle*. Berkeley might have said, as some contemporary analytic philosophers have said explicitly, that the meaningfulness of a word does not presuppose the existence of an entity which is *what* the word means. That the word *triangle* is unambiguous in the context of geometric discourse does not entail that it functions in that context as a *name* of a unique entity, call it a "universal." One chief source of Platonism, according to this point of view, is the naive and mainly unconscious assumption that every meaningful word is a name of something. But the verb "to mean" is, in the relevant respect, more like the verbs "to dream" and "to wish" than like the verbs "to name" and "to eat"; there must *be* something that can be named in order that an act of naming may take place, and there must *be* something that can be eaten in order that a process of eating may occur. But would it not be grotesque if I argued to the existence of green dogs that can sing Verdi arias from the fact that I dreamed of such an animal, or to the existence of a woman who is my wife and combines more virtues than any other woman from the fact that I wish for such a wife? In grammatical terminology, this criticism of Platonism accuses it of construing "to mean" as a transitive verb, whereas this verb is obviously intransitive.

The chief philosophic motive behind Bertrand Russell's *theory of descriptions* (*3*) was to expose this confusion and thereby to inhibit the tendency to postulate entities that do not exist in the physical universe. To borrow Russell's own example, consider the sentence "the present king of France is bald." On a crude level of analysis one might say that, since the sentence is meaningful (all constituent words are meaningful and are arranged in a syntactically correct way), it must be about something. And what is it about if not the present king of France? But there is no present king of France! Is it, then, about a Platonic essence or Idea that happens to be unexemplified in the physical world? Russell's answer was that no such metaphysical postulations are required to account for the significance of the sentence. We have only to distinguish between *contextual meaning* and *denotation*: a definite description, like "the present king of France," has meaning in context but does not denote anything at all. It has meaning in the context of sentences that contain it, and this meaning can be explained by means of a synonymous sentence in which no definite descriptions occur: "there is one and only one individual which is present king of France, and that individual is bald" (*4*). Once meaning and denotation are thus distinguished—a symbol may be meaningful (in context) without denoting anything—the argument for the existence of universals from the meaningfulness of predicates collapses, for a predicate can be meaningful in the context of entire sentences without being a name of anything.

This, in fact, is a fundamental tenet of contemporary semantic nominalism (I call this philosophic school "semantic" nominalism to distinguish it from metaphysical nominalism, because its members tend to avoid such metaphysical locutions as, "only particular things or events in space and time are real, universals are abstractions that have no reality"): to this school, predicates are contextually meaningful, and to explain the meaning of a predicate is to formulate a rule for applying the predicate to particular objects, but predicates are not names

of anything. The predicate "blue" is not related to its meaning in the way that the name "John" is related to John. Indeed, it is already misleading, the semantic nominalists urge, to speak of the meaning of "blue," since this expression suggests an elusive entity. It is preferable to speak of the rule governing the use of "blue," and to give this rule is to formulate the conditions under which it is correct to apply "blue" to a thing. In the case of "blue," the rule of usage cannot be formulated without pointing at particulars. In this respect the rule of usage for, say, "mermaid" is, of course, of a different sort, since we can be said to understand what "mermaid" means, although we have never encountered any mermaids. But, the semantic nominalist insists, to grasp this meaning is not to be face to face, as it were, with a universal, call it mermaidhood, but just to know under what conditions it would be correct to apply the predicate "is a mermaid" to an object (*5*).

An important application of the theory of contextual meaning to mathematics arises if one reflects on the meaning of infinitesimals, expressed by "dx," "dy," and so on. Mathematicians used infinitesimals successfully long before they were clear about their meaning. At first glance there is an air of hocus-pocus about infinitesimals: how can a genuine quantity be neither zero—insofar as an infinite sum of infinitesimals equals a finite quantity—nor finite—insofar as a finite sum of infinitesimals does not equal a finite quantity? And how can the ratio of dy (where y is a function of x) to dx equal a finite number if neither dx nor dy are finite numbers? The differential quotients themselves seemed perfectly legitimate, since they admitted of geometric interpretation, namely, as measures of the slopes of tangents. But what troubled both mathematicians, who used the device of infinitesimals without being able to justify it "philosophically" (as contrasted with "pragmatic" justification: it works!), and philosophers, who were critical of the calculus (Berkeley, for example), was that the components of the differential quotient had no intelligible meaning. The theory of contextual meaning, however, justifies the use of infinitesimals as "incomplete symbols," in Russell's phrase—that is, as symbols which do not denote any funny kinds of numbers but have meaning in context. An equation of the form "$\mathrm{d}y/\mathrm{d}x = c$" means that the limit approached by a sequence of difference quotients $\triangle y/\triangle x$, as $\triangle x$ becomes smaller and smaller, equals c, and the meaning of "limit" can be explained without postulating any numbers other than finite ones. When one has explained the meanings of equations that involve differential quotients, both such as equate them to a constant and such as express them as a function of a variable, one has explained the contextual meaning of differential quotients and of their components.

Classes, Attributes, and the Logical Analysis of Mathematics

It was stated in the beginning of this article that it is difficult to avoid Platonism if one becomes philosophic at all and asks what a number, as distinct from a numeral, is. It seems that a number is either a universal—that is, a common property of classes of discrete objects that can be set into one-one correspondence—or else a *class* of such "similar" classes. The number two in particular, then, is revealed as either the universal "twoness" or the class of all pairs. Although classes are, for good or for bad reasons, somehow more acceptable to nominalists than are universals (or attributes, as we shall henceforth call them), they are likewise abstract entities. By "the class of all men," for example, a logician does not mean a group of men that could possibly be seen but the conceptually apprehended totality of men past, present, and future (*6*).

Can these Platonistic temptations be overcome by means of the theory of contextual meaning? That is, can it be maintained that numerals have only contextual meaning and do not denote any entities at all? Now, contextual definitions of the natural numbers in terms of logical constants and nonnumerical variables can be constructed, as was shown by Frege and Russell (*7*) independently (although Frege has historical priority and influenced Russell's thinking on the foundations of arithmetic); and these contextual definitions clarify, in a very important way, the language of *applied* arithmetic.

Let us being with the first natural number, zero. What is meant by a statement like "the number of French judges in the Supreme Court is zero"? Obviously, it means that there are no such persons, or that the class of such persons is empty. Here, "not," "there are," and whatever symbol is used to denote membership in a class are logical constants. Note that zero, just like the larger numbers, is predicable of classes, not of individuals; that is, statements of the form "there is nothing of the kind K" are meaningful, but statements of the form "this thing is not" or "this thing is nothing" are not.

Next, consider the number one. Again, we cannot predicate unity in the arithmetical sense of an individual: "the sun is one" makes no sense; "there is one (and only one) sun" makes sense. That is,

we might say that the class of solar objects has exactly one member, and this means, according to the Frege-Russell analysis, that there is an x such that x is a member of the class of solar objects, and for any y, if y is a member of the class of solar objects, then y is identical with x. Similarly, "there are two things of kind K" means that there is an x and a y, y distinct from x, such that x and y are of kind K, and for any z, if z is of kind K, then z is identical with x or with y, and so on for the larger numbers. It should be noted that the contextual definition of the arithmetical predicate of classes "one" is not circular, because "there is *an* x" does not have to be read as "there is at least *one* x" but may be read as "it is not the case that there is no x."

Are these definitions correct? Do they formulate what we mean by arithmetical symbols in the context of applied arithmetic? This question cannot be answered unless criteria are specified which must be satisfied by correct definitions. Now, an important criterion of correctness which these contextual definitions can be shown to satisfy is that the equations of arithmetic which are actually used in deducing numerical statements about empirical subject matters from other numerical statements should be logically demonstrable. For example, we would expect from adequate contextual definitions of "2," "3," and "5" that they make possible a formal proof (that is, a proof based on nothing but laws of logic) of the proposition "if a person has 2 daughters and 3 sons, then he (or she) has 5 children." It may be thought that this proposition of applied arithmetic is just a special case of the general equation "$2+3=5$" and that there is no further problem, since the equation itself is simply "true by definition." But insofar as the equation is just a provable formula in an uninterpreted system of arithmetic, its constituent arithmetical symbols do not have the sort of meaning that is presupposed by empirical applicability. In such a system (a "formal calculus," in the terminology of the logicians) 1 is defined as the successor of 0, 2, as the successor of 1, 3, as the successor of 2, and so on, where 0 and *successor* are primitive terms. These primitives also enter into the recursive definition of $+$: $x+y'=(x+y)'$; $x+0=x$ (here, "$\ldots'$" is short for "the successor of . . . ."). In terms of these definitions, and of the rule that equals are interchangeable, it is indeed possible to transform $2+3$, step by step, into 5, but as long as the primitives remain uninterpreted, the defined symbols likewise are without interpretation. The definition $2=1'$, which occurs within the formal calculus, does not enable one to decide whether a given class has two or more or less members; indeed, there is nothing within the formal calculus itself to suggest even that numerals designate properties of classes that can be ascertained by counting.

We require, then, an *interpretation* of the arithmetical primitives in terms of an understood vocabulary before we can even significantly raise the question of whether the equations are *true*. And if we want to justify the use of $2+3=5$ as a rule for deducing "x has 5 children" from "x has 2 sons and 3 daughters" (with the additional help of the definition or theorem "x is a child of $y=x$ is a son or daughter of y"), we have to assign to the numerals just those meanings which they have in such *empirical* contexts as "x has two sons." This is precisely what the Russellian contextual definitions in terms of logical constants accomplish; they at once clarify the meanings of empirical statements in which numbers are applied and justify the belief that "x has 5 children" *logically follows* from "x has 2 sons and 3 daughters."

However, as both Frege and Russell clearly saw, such contextual definitions are insufficient for the elucidation of numerical symbols. In effect, what has been defined so far is only the statement form "class A has n members." But we still do not know what 2, for example, means in the context "2 is a prime number." Here, 2 does not occur as predicate but as subject. This is the kind of consideration that led Frege and Russell to ask *what the number 2 itself is*. Formally speaking, in the context "2 is a prime number," the symbol "2" is a *name*, not an incomplete symbol. What is it the name of? Two alternative answers may be considered: (i) the class of all classes with two members, (ii) the attribute of being a class with two members. Answer i is a special case of Russell's general definition of numbers as classes of similar classes. Two classes are said to be similar if there is a one-one relation which relates every member of one class to a member of the other class, and a relation R is one-one if at most one entity has R to a given entity and a given entity has R to at most one entity (for example, being the immediate successor of; being the positive square root of; being the wife of, in a monogamous society).

It is natural to think of a number as *the common property* of all mutually similar classes. But this definition is open to the objection that there is no guarantee that a set of similar classes have just one property in common (*8*). For example, if all individuals have a certain property f in common, then all possible pairs that could be formed out of them would have the common property "containing as members individuals with property f." By the foregoing definition, therefore, the number 2 would fail to be unique. On the other hand, the *class* of

all pair-classes is necessarily unique, and this is why Russell preferred to define numbers as classes of similar classes.

Nevertheless, there is at least one reason why it may be better to regard numbers as special kinds of attributes, in the sense of definition ii. The formal difference between classes and attributes is that attributes which apply to exactly the same entities may still be distinct, whereas classes with the same membership are identical. Thus, we can conceive of a universe in which all round things are blue and all blue things are round. If such were the world, the class of blue things would be identical with the class of round things, yet the attributes blueness and roundness, the one a color the other a shape, would remain perfectly distinct. Or, to illustrate by a mathematical example, the class of natural numbers between 1 and 3 is the same as the class of even primes, but the corresponding attributes are clearly different; we describe the number 2 differently if we describe it as the natural number that is less than 3 and greater than 1, or as the only even number that is a prime number. Again, many distinct attributes are not possessed by anything at all: being a unicorn, being a blue dog that can speak French, being a golden mountain, and so forth. But they all correspond to the same class, the *null* class. There cannot be more than one null class, for if there were two, one would have to have a member which the other does not have, and this contradicts the hypothesis that neither has any members.

Now, to come closer to the defect of definition i, which is of interest here, the arithmetical meaning of the term *successor* (as used, for example, in the definition of 1 as the successor of 0) is such that necessarily distinct numbers have distinct successors. This, in fact, is one of five postulates upon which the Italian mathematician and logician Peano erected the arithmetic of natural numbers. And one of Russell's avowed aims in "logicizing" the concepts of arithmetic was to so define Peano's primitives ("zero," "successor," "natural number") that all the postulates (and therewith the theorems) of uninterpreted arithmetic turn into logically necessary propositions—propositions that are formally deducible from the purely logical axioms of *Principia Mathematica* (*9,* chap. 1–3; *10*). Russell noticed, however, with considerable intellectual discomfort, that his definitions of natural numbers as classes of similar classes of individuals do not make Peano's axiom that distinct numbers have distinct successors logically necessary. For suppose (what is conceivable without self-contradiction) that the number of individuals (*11*) in the universe were some finite number n. Then the number $n+1$, defined as the class of all classes with $n+1$ members, would be the null class, since there would be no classes with $n+1$ members. For the same reason, $n+2$ would be the null class. Hence, we would get $n+1=n+2$, although n, being a nonempty class, is distinct from $n+1$. For reasons connected with the theory of types (a theory devised as a solution of logical paradoxes, which we cannot and need not explain here), Russell saw no other escape from this difficulty than to postulate that the number of individuals is not finite ("axiom of infinity") (*9,* chap. 13).

Now, we have seen that $n+1=n+2$ follows from the assumption that the number of individuals is n, together with the Russellian definition of numbers as *classes* of similar classes. But if the number $n+1$ is, instead, defined as the *attribute* (of a class) of having $n+1$ members, Russell's conclusion does not follow. For though, on that assumption, both attributes would be empty (inapplicable), they would remain just as distinct as, say, the attributes of being a mermaid and of being a golden mountain, and for just the same reason: they are defined as incompatible attributes. For example, the expressions "*A* has two members" and "*A* has three members" are so defined in *Principia Mathematica* (see the foregoing sample contextual definitions) that it would be contradictory to suppose that the same class had both (exactly) two and (exactly) three members. Therefore, even if just one individual existed, the number 2 would retain its conceptual distinctness from the number 3; and this argument, obviously, can be generalized for any finite number n and its successor.

Apart from the question of the axiom of infinity, it is, within the framework of the logical analysis of mathematics contained in Whitehead and Russell's monumental *Principia Mathematica,* unimportant whether numbers be conceived as classes (of classes) or as attributes (of classes), for names of classes are, in either case, contextually eliminable, according to Russell's theory of classes as "incomplete symbols." According to this theory, a statement that ascribes an attribute F to a class that is defined as the totality of entities that have attribute G is analyzed as follows: there is a predicative function θ such that θ is formally equivalent to G and such that θ has F (*12*). In this context, *function* means, not numerical function, but propositional function, and for the present purposes propositional functions may be identified with attributes. Two attributes are "formally equivalent" if they apply to the same entities, and a predicative function is an attribute that does not presuppose, in its definition, a totality of attributes.

For a full understanding of this theory of classes, familiarity with the theory of types, especially the "ramified" theory with its distinction between the

type and the order of a function, would be required (*13*). But for the present discussion, all that matters is that in the "primitive" notation of *Principia Mathematica*, no names of classes but, instead, variables ranging over attributes (propositional functions) occur. For a semantic nominalist holds that the language of mathematics involves an obscure Platonism as long as there are in its primitive notation either names of, or variables ranging over, abstract entities; and he regards attributes as abstract entities (*14*). Take, for example, the simple statement that two individuals, x and y, differ in at least two respects. This means that there is an attribute f and an attribute g such that f is not equal to g and such that x has f and y does not have f and x has g and y does not have g, or y has f and x does not have f and x has g and y does not have g, and so on. But here f and g are variables ranging over attributes; therefore such an analysis is unacceptable to a semantic nominalist.

Now consider the more complicated statement: $2 + 2 = 4$. The logical analysis performed by Frege and Russell shows that we do not have to regard the symbols "2" and "4" as names of mysterious entities. Even the names "the class of all classes with two members" and "the class of all classes with four members" are, in principle, eliminable. But what cannot be got rid of, apparently, are variables ranging over attributes. For if, in conformity with Russell's view that classes are logical fictions, not real entities, we define numbers as certain kinds of attributes *of attributes* (in the sense in which "being a color," for example, is an attribute of attributes, namely, of blueness, redness, and so on), we obtain the following analysis of $2 + 2 = 4$: for any attributes f, g, h, if just two individuals have f and just two individuals have g and nothing has both f and g, and if an individual has h if and only if it has either f or g, then just four individuals have h (illustration: f = being a coin in my left pocket at time t_0, g = being a coin in my right pocket at t_0, h = being a coin in one or the other of my pockets at t_0).

Is mathematics, then, irremediably Platonistic? Or is there some way of eliminating attribute variables so as to satisfy the demands of semantic nominalism? The issue is a highly technical one, but the following suggestion should be intelligible to those without any knowledge of symbolic logic. As was explained in earlier paragraphs, the nominalist insists that a predicate can be meaningful without being the name of anything. For this reason he refuses to transcribe (i) "x is red" into (ii) "x has the attribute redness." From ii we may deduce (iii) "there is an attribute f such that x has f," but since "is red" has meaning only in context, it is not a name of a value of a variable. The deduction of iii from i is as illegitimate as the deduction of (iv) "there is something which I am now thinking of" from (v) "I am now thinking of a unicorn." The transition from v to iv is tempting because it is similar to the perfectly valid deduction of (vi) "there is something which I am now eating" from (vii) "I am now eating an apple." But, whereas we could not eat apples if there were no apples, we can think of unicorns in spite of there not being any. If we allow the deduction of iv from v, we get entangled in a verbal contradiction, since the something which iv says I am thinking of is, of course, a unicorn, and then there must be unicorns after all! To resolve the contradiction, the Platonist may say that the object of my thought is the attribute of being a unicorn, not a concrete unicorn. The nominalist, on the other hand, criticizes the transition from v to iv on the ground that "unicorn" is not a name but a predicate (or part of the predicate "is a unicorn"). Correctly analyzed, v, unlike vii, does not assert a relationship between two independently existing entities but simply characterizes my thought as what we might call a "unicornish" thought. Similarly, "this is a picture of a unicorn" characterizes the picture as unicornish; it does not mean that this has the relation *being a picture of* to an independently existing entity—in the way that this is the meaning of "this is a picture of my son."

Bearing in mind the distinction between predicates (which may not be replaced by variables ranging over some kind of entities) and names, let us return to the logical analysis of $2 + 2 = 4$. To satisfy the nominalist, we must replace "have f," where f represents a name of an attribute, with "are f." But then f, g, and h become letters that represent predicates, not names of attributes; hence, they are not variables over which we may "quantify," as the logicians say. The prefix (quantifier) "for any attributes f, g, h" must, accordingly, be cut off, for the letters f, g, and h do not represent names of attributes at all.

What, then, is the law of arithmetic $2 + 2 = 4$ in logical and, at the same time, nominalistic interpretation? The surprising answer is that there is not *one* such law, there is not a sentence containing, besides variables, only logical constants which we can point to and say "this sentence expresses the arithmetical law that 2 and 2 make 4," for "if just two individuals are f and just two individuals are g . . ." is a schema, not a statement; it does not express a proposition at all. In order to obtain propositions out of it, we must substitute specific predicates for the schematic letters in it, and there are as many such propositions as there are triplets of predicates applicable to individuals. If the lan-

guage of arithmetic is reconstructed nominalistically, then, no laws of pure arithmetic can be formulated in the object-language—that is, the language which, whatever it may refer to, does not refer to symbols. Instead, one has to formulate in the meta-language (the language that is used to talk about the object-language) statements to the effect that any substitution instance of such and such a schema is logically true.

This conclusion, of course, applies not only to the laws of arithmetic and of higher mathematics but also to the basic laws of logic. For example, consider the law of medieval logic, "what is true of all is true of any," which, in symbolic logic, reads as follows: for any y, if all x's have attribute f, then y has f. A Platonistic logician can refer to *one* logical truth expressed by the sentence: for any attribute f and for any individual y, if all x's have f, then y has f. A nominalistic logician must ascend to the meta-language and there declare that every statement of the form "for any y, if all x's are f, then y is f" is logically true (*15*).

What Do the Ontological Questions Mean?

To what extent a nominalistic reconstruction of mathematics is feasible is certainly an interesting technical problem that deserves investigation in its own right. But attempts at such reconstruction are usually motivated by a philosophic prejudice about what "really exists." Being asked why he wants to get rid of variables ranging over attributes, the nominalist is likely to reply that he cannot admit the existence of attributes because the very notion of "attribute" is obscure to him. But even if the notion could be clarified to his satisfaction, he would probably remain reluctant to admit the existence of such entities. Thus, nominalists usually concede that the notion of "class" is quite precise, and yet they cannot countenance classes as "real entities."

The main argument in support of the accusation that the meaning of the word *attribute* (and of its synonyms in philosophic literature: universal, property, intension of a predicate) is obscure is that it is not clear under which conditions two expressions can be said to designate the same attribute (*16*). For example, is the attribute (of a number) of being equal to the product by itself the same as the attribute of being the successor of zero? Is the attribute of being a rectilinear closed figure with three sides the same as the attribute of being a rectilinear closed figure with three interior angles? We have here predicates which are interdeducible (logically equivalent), but it seems debatable whether they are *synonymous*. And the question of the criterion of synonymy is, indeed, difficult and highly controversial in contemporary semantics (*17*). The same nominalists who frown on attributes find it easier to accept talk about classes precisely because the condition of identity of classes is clear and uncontroversial: A and B are the same class if, and only if, they have the same members.

Now, this particular argument against "Platonistic" discourse seems to me highly illogical. If "attribute" is held to be obscure on the ground that the identity condition for attributes is not clear, then one would expect that all is well with the identity condition for individuals—that is, those concrete entities, whatever they may be (material particles, observable things, qualitatively distinguishable events, space-time points), which alone really exist according to nominalist ontology. Yet, the identity condition for individuals that is usually accepted is Leibniz' principle of the "identity of indiscernibles," which in modern logic is formulated thus: $x = y$ if, and only if, for every attribute f, x has f if and only if y has f. Clearly, if such is the definition of identity of individuals, and a given kind of entity is deemed obscure to the degree that its identity condition is not clear, then the alleged obscurity of "attribute" must infect the term *individual* too.

Two replies are open to the nominalist: (i) Since we realize that an explicit definition of identity requires quantification over an attribute variable, we do not explicitly define this logical constant at all. Instead, we lay down an axiom schema for identity which does the same job as the explicit definition. It says, in effect, that identicals may be substituted for each other without change of truth-value of the sentence into which the substitution is made, and contains a schematic predicate letter, not a quantifiable attribute variable: if Fx, and $x = y$, then Fy. (Note that, in accordance with the explanation given earlier, a single axiom or definition is thus replaced by a family of axioms of the same form, namely, those that result from the substitution of a predicate for the schematic letter F.) (ii) Identity is explicitly definable with the help of a *class variable*; hence, the intelligibility of "individual" exceeds that of "attribute" by at least as much as the latter is exceeded by the intelligibility of "class": $x = y$, if and only if x and y are members of just the same classes of individuals.

My rejoinder to i is that an exactly similar axiom schema can be laid down for identity of attributes. That is, it is postulated that from "f is F" and "$f = g$" we may deduce "g is F." Here F is a schematic letter to be replaced by predicates that are applicable to attributes, not to individuals (for example, "is a color," "is possessed by exactly two individuals," "is a desirable attribute"). Con-

sequently, attributes and individuals are still in the same boat with regard to the alleged obscurity of the identity condition. As rejoinder to ii, obviously just the same kind of explicit definition of identity of attributes can be given. Attribute *f* is the same as attribute *g* if, and only if, *f* belongs to just the same classes of attributes that *g* belongs to.

The argument from the identity condition, therefore, does not establish the superior intelligibility of nominalistic discourse. Independent arguments are needed. Perhaps the basic argument is the simple "common-sense" argument that the only way of proving the existence of anything is sense perception or inference from perceived things or events to unperceived things or events, in the manner of the physical scientist. But attributes cannot be perceived by the senses, nor does the postulation of the existence of attributes explain the nature of the perceived world in the way in which the theory of electrons, say, explains observed phenomena.

Nevertheless, the assertion that attributes are "abstract," not given in sense experience, should not be uncritically accepted. There are many simple qualities which *recur* in our sense experience and which, by this criterion, are universals. We see the same shade of blue in different patches or in different parts of the same patch, hear the same pitch in different sounds, taste the same taste in different portions of the milk we drink. It is likewise artificial to deny that we have direct experience of *relational* universals—for example, of the relation of temporal succession and the relation of spatial proximity. The counterargument that it is not, after all, certain that precisely the same quality recurs presupposes that one understands what it would be like to perceive exactly the same quality in different contexts. But since the expressions "exactly the same shade of blue," "exactly the same pitch," and so on, can be explained only *ostensively* (*18*) (you point, say, to adjoining parts of the same expanse and say "see, they, for example, are the same shade of blue—that is what I mean by 'same shade of blue' "), this argument is really self-defeating. At any rate, the argument from the limits of sensory discrimination is hardly relevant to the question of the existence of mathematical attributes. For (natural) numbers may be regarded as attributes of attributes of individuals; that is, the number *n* is the attribute (of an attribute) of having *n* instances, and to deny that two attributes *f* and *g* have the same number is to deny that their instances can be matched one by one. But surely there are attributes that have the same number in this sense—for example, being a finger on my left hand and being a finger on my right hand. And if there are attributes that have the same number, does it not follow that there are attributes? What, then, is the argument between the nominalist and the realist all about?

Indeed, it is difficult if not impossible to attach any sense to the "ontological" question about whether there are attributes (*19*). If someone asks me this question, my natural reaction is, "why, of course there are: blueness, roundness, hardness, twoness, and millions more." If he denies that these are examples of attributes, then he cannot mean by *attribute* what is ordinarily meant by the word, for one explains its meaning in terms of just such examples before entering into the more technical explanation of what, in general, distinguishes attributes from other types of entities, like individuals, or classes, or propositions. To deny that roundness is an attribute would be as absurd as to deny that 5 is a number, or that parenthood is a relation, or that the desk I am writing on is a thing. But if it is not denied, then one cannot deny, either, that there are attributes, since, according to elementary logic, any statement of the form "*a* is *f*" entails "there is an *x* (at least one) such that *x* is *f*." In the terminology of some contemporary analytic philosophers of language, the ontological statement is really a trivial *analytic* statement that cannot be denied if one understands the intended meaning of the type designation, like *attribute, relation, class,* and so on (*20*).

To be sure, if one cannot countenance entities other than particular things because one cannot understand how something can "be" without having a tangible and visible existence at a particular place (one at a time!), then one's intellectual discomfort is just self-imposed through the fallacy of "reification": one is just naively associating with the expression "there are" images of spatially located things. One might, for example, be made intellectually uncomfortable by a mathematician's statement that the number 0 exists. According to the Russellian class conception of numbers, the number 0 is a very peculiar class: the unit class whose only member is the null class. But how can a class be said to exist unless it has an existing member? And is not the null class, by this very criterion of class existence, nonexistent? Yet, in the sense of "there is" which is implicitly defined by the entailment (which was given in the last paragraph), "if *a* is *f*, then there is an *x* such that *x* is *f*," the statement "there is a class which is the null class" is just a trivial consequence of the logically true statement, "there is nothing which is distinct from itself" (in class terminology, the class of things that are not self-identical is null) as well as a trivial consequence of any logically true statement of the form "there is nothing which both is and is not *f*." The puzzle about how such shadowy, ghostly entities as the null class, and therewith the

number 0, can exist arises only because one associates with the expressions "there is" and "there exists" images of spatial location, although they function purely as *logical constants* in the context of mathematical existence assertions: "there is something which is *f*" means no more nor less than "not everything is not *f*."

Most existential statements that occur in everyday discourse refer to physical objects or events that exist, or occur, at some place and at some time —or at some place-time, in relativistic language. From this derives the habit of associating with the logical constant "there is" and its variants, images of spatial localization. It is only these associations, and the failure to understand the purely logical nature of such existential statements as "the null class exists," "there are classes," "there are attributes," which produces intellectual discomfort.

To be sure, some mathematicians and logicians reject an existential statement "there is an x such that x is *f*" as meaningless in a case where it cannot be proved by deduction from a provable corresponding singular statement "*a* is *f*." They are sometimes called "intuitionists," sometimes "finitists." They reject merely indirect proofs by the *reductio ad absurdum* method: there is an x such that x is *f*, because if you assume that there is no such x you get involved in a contradiction, whence it follows that the assumption is false; and if the contradictory of a proposition p is false, then p is true (law of the excluded middle).

A well-known application of this "finitist" methodology of mathematics is the rejection of Zermelo's axiom of choice. This is the following existence assertion: for any class K of mutually exclusive and nonnull classes, there is a class which contains exactly one member from each member of K ("multiplicative class"). Now, in order to prove that a given class is a multiplicative class with respect to K, one must define it in terms of a method of selection of its members from the members of K. For example, let K be the class of all mutually exclusive pairs of integers. Here, a multiplicative class is easily defined as the class to be constructed by picking, say, the smaller integer from each pair. On the other hand, consider the class K which is formed as follows: we first take the class of proper fractions between 0 and 1, then the class of proper fractions between 1 and 2, then the class of proper fractions between 2 and 3, and so on. Obviously, the members of K are classes that have no members in common, since the integers that delimit the intervals are not themselves proper fractions. But now it is a little less obvious that one can describe a method of constructing a multiplicative class. The classes from which the members of the multiplicative class are to be selected have neither a first nor a last member, since between any proper fraction and the "closest" integer, there is another proper fraction. One might think of representing each class by its "mid-point," perhaps—that is, by that fraction which equals the arithmetic mean between the integral limits. But this method of selection becomes unavailable for the following class K: the class of proper fractions between 1/2 and 3/2, limits excluded, then the class of proper fractions between 3/2 and 5/2, limits excluded, and so on. For here the "mid-points" are integers, not proper fractions. Perhaps there is still a method of selection with respect to this K, but do we have any guarantee that for *any* class of mutually exclusive nonnull classes a method of constructing a multiplicative class can be described? Certainly not.

Russell mentions, in his *Introduction to Mathematical Philosophy* (*9*, p. 126), the class of all pairs of socks in order to illustrate that it is not self-evident that there is a "selector" for defining a multiplicative class with respect to a class of mutually exclusive nonnull classes. A multiplicative class, here, could be defined only if there were a property which just one sock in each pair possessed, say being the left sock, or being the red sock, and so on. The finitist concludes that it is meaningless to assert the axiom of choice in complete generality. He holds that it is meaningless to assert that *there is* a multiplicative class with respect to a given class of classes unless one can give a rule for constructing one. And this fits into his general methodological requirement that existence theorems be proved "constructively"—that is, by exhibiting an instance, or a rule for constructing an instance, that satisfies the existence theorem (*21*).

But, however the methodological dispute between finitists and "realistic" mathematicians who wish to continue using nonconstructive proof methods (such as the *reductio ad absurdum* method by which Euclid discovered the irrationality of the square root of two) may be resolved, it really has no bearing on the ontological pseudo-questions, "are there classes?" "are there attributes?" and so on. For suppose we accept the finitist interpretation of "there is an x such that x is *f*" as meaning "there is at least one constructively provable statement of the form 'x is *f*'," where x ranges over abstract entities like numbers or classes. What should prevent one who has a penchant for asking ontological questions from asking whether there really are such abstract entities? The very asking of a question of the form "is there a number with property *f*," he might say, presupposes that *there are numbers* (*22*), whatever proof technique one may use to answer it. This presupposition, however, can be established quite easily by citing examples of

numbers. To be sure, no philosopher who regards the existence of numbers and other abstract entities as problematic would be satisfied with the ironical answer: "Of course, there are numbers: 2, 5, 7, and many, many others." But the trouble with the ontological question is just that no method of finding the answer has ever been described by those who keep asking it. And this means not that we do not know the answer but that we do not understand our own question.

There is but one interpretation of the ontological question that makes it both intelligible and interesting, and this is an interpretation that turns it into that semantical-logical question which was touched on earlier in this article. What exactly did Russell mean by the assertion that classes are not real entities but are just "incomplete symbols"? The best way to discover the meaning of a statement is to look at the reasons offered in support of it. Now, Russell's reason for denying the reality of classes was that statements that contain names of classes or class variables are satisfactorily translatable into a symbolism which does not contain such expressions. (To take a simple example: "the class of dogs is included in the class of animals" means "for any x, if x is a dog, then x is an animal.") I propose to identify this *reason* for the denial of the reality of classes with the *meaning* of the denial. Similarly, the nominalist who denies the reality of any kind of abstract entities may be interpreted as affirming that the meanings of statements that contain names of, or variables ranging over, abstract entities can be analyzed by means of a language that satisfies nominalistic requirements. Whether a nominalistic language is clearer, more intelligible, than a realistic language is, of course, debatable; indeed, this may be a matter of taste, of arational preference. But it must be admitted, I think, that the modern semantical interpretation of the time-honored (or time-dishonored) nominalism-realism dispute has the merit of making it scientifically meaningful, although it may be countered that, for this very reason, it is misleading to call this modern, semantical-logical issue by the same name because one thus seems to accuse those who prefer the one or the other type of language of a subconscious addiction to realistic or nominalistic "ontology."

References and Notes

1. See, for example, W. P. Montague, *The Ways of Knowing* (Macmillan, New York, 1953), chap. 4; B. Russell, *The Problems of Philosophy* (Oxford Univ. Press, 1943), chap. 9.
2. G. Berkeley, *A Treatise Concerning the Principles of Human Knowledge*, sec. 18.
3. It was first formulated in "On denoting" [*Mind* (1905)], reprinted in Feigl and Sellars, *Readings in Philosophical Analysis* (Appleton-Century-Crofts, New York, 1949). See also B. Russell, *Introduction to Mathematical Philosophy* (Allen and Unwin, London, 1948), chap. 16.
4. Of course, "France" is, in turn, an abbreviation for some definite description, but Russell believed that by repeated application of his rule of translation, any definite description could eventually be eliminated.
5. A programmatic formulation of this semantic theory, which derives from L. Wittgenstein and M. Schlick, is as follows: the meaning of an expression consists in the rules for its use. Wittgenstein, a dominant member of the "Vienna Circle" (the founders of logical positivism) [see V. Kraft, *The Vienna Circle*, A. Pap, Transl. (Philosophical Library, New York, 1953)], exerted a powerful influence on English analytic philosophy. For a lucid application of Wittgensteinian semantics to the problem of universals, see M. Lazerowitz, "The existence of universals" [*Mind* (1946)], reprinted in *The Structure of Metaphysics* (Routledge and Kegan Paul, London, 1955).
6. There is, incidentally, a neat proof that classes are different from wholes, whether the parts of the whole be spatially or temporally continuous or discrete. If x is part of a whole y and y is part of a larger whole z, then x is part of z. But if x is a member of class y and y is a member of class z, then x is not a member of z. For example, John is a member of the class of men, the latter is a member of the class of classes of living organisms, but John is not a member of the latter class, since he is not a class. Furthermore, the same whole may be conceptualized as different classes: we can consider an organic body, for example, as a composite of cells but also as a composite of molecules. We then have a composite of cells that is identical with a composite of molecules; but a class of molecules cannot be identical with a class of cells, for, since cells are different from molecules, nothing can be a member of both classes.
7. For an elementary exposition of "logicism" (as the reduction of mathematics to logic, attempted by Frege and Russell, is called), see C. G. Hempel, "The nature of mathematical truth," reprinted in Feigl and Sellars (*3*) and in Feigl and Brodbeck, *Readings in the Philosophy of Science* (Appleton-Century-Crofts, New York, 1953).
8. See B. Russell, *The Principles of Mathematics* (Norton, New York, ed. 2, 1938), par. 110.
9. See B. Russell, *Introduction to Mathematical Philosophy* (*3*).
10. See C. G. Hempel, "The nature of mathematical truth" (*7*).
11. The concept "individual" is but negatively defined in *Principia Mathematica* (*12*): anything that is neither a class nor an attribute nor a proposition. The question is thus left open whether the individuals are observable things, or postulated particles, or physical events, or what not.
12. See A. N. Whitehead and B. Russell, *Principia Mathematica* (Cambridge, England, 1910–13; ed. 2, 1925–27), vols. 1–3, introduction, p. 71f.
13. See *12*, introduction, chap. 2. More easily intelligible expositions of the ramified theory of types are Lewis and Langford, *Symbolic Logic* (Century, New York, 1932), chap. 13; I. Copi, *Symbolic Logic* (Macmillan, New York, 1954), appendix B.
14. What I call "semantic nominalism" has been formulated by W. V. Quine, in "Designation and existence," reprinted in Feigl and Sellars (*3*), and in "On universals," *J. of Symbolic Logic* **12**, No. 3

(1947). See especially *From a Logical Point of View* (Harvard Univ. Press, Cambridge, Mass., 1953), chaps. 1, 6. The program of nominalistic reconstruction of mathematics is sketched by W. V. Quine and N. Goodman in "Steps towards a constructive nominalism," *J. of Symbolic Logic* **12**, 105 (1947).

15. Subtle problems emerge once one asks whether, granted that the object-languages of logical and mathematical systems can be constructed in accordance with nominalistic restrictions, the meta-language also could be nominalistic. For example, surely we mean to assert the logical truth of *all possible* substitution instances of a given schema, not just of those that are actually written down somewhere and at some time. But it is even doubtful that the method described in the text makes possible a nominalistic rewriting of mathematical *object*-languages, for quantifiers that refer to attribute variables within the scope of another quantifier cannot be eliminated by such a simple device. The statement "there is no largest number" illustrates this complication if numbers are construed as attributes of attributes: for any attribute f, if f is a number-attribute of an attribute, then there is a number-attribute (of an attribute) g such that g is larger than f.
16. This argument has been used chiefly by W. V. Quine. See "Two dogmas of empiricism," reprinted in *From a Logical Point of View* (*14*), and "The problem of interpreting modal logic," *J. of Symbolic Logic* **14**, 43 (1947).
17. See N. Goodman, "On likeness of meaning," in *Semantics and the Philosophy of Language*, L. Linsky, Ed. (Urbana, Ill., 1952); B. Mates, "Synonymity," *ibid.*; A. Naess, "Toward a theory of interpretation and preciseness," *ibid.*; G. Frege, "On sense and nominatum" in Feigl and Sellars (*3*); R. Carnap, "Meaning and synonymy in natural language," *Philosophical Studies* (Apr. 1955), reprinted in *Meaning and Necessity* (Chicago, ed. 2, 1956), appendix; W. V. Quine, "Two dogmas of empiricism" (*16*).
18. It may, indeed, happen that a appears the same color as b, b appears the same color as c, yet a is distinguishable in color from c. Such cases suggest the following general definition of qualitative identity in terms of indistinguishability: x and y have the identical quality Q if, and only if, anything which is indistinguishable with respect to Q from x is also indistinguishable with respect to Q from y. But then, "indistinguishable (with respect to Q)" is the term that must be ostensively defined, and it will be self-refuting to deny that *sensory appearances* (many philosophers speak of "sense-data") exhibit genuine universals.
19. The chief advocate of the logical positivist view that such ontological questions are "pseudo-questions," devoid of cognitive meaning (a view I strongly incline to myself), is R. Carnap. See his "Semantics, empiricism, and ontology," reprinted from the *Revue Internationale de Philosophie* (1950) in *Semantics and the Philosophy of Language* (*17*).
20. It is often overlooked by philosophers that a statement which is analytic in the sense that it cannot be seriously denied by one who understands it does not have to express a necessary proposition—that is, a proposition that cannot be conceived to be false. If this *is* overlooked, then one can easily refute the argument in the text by an analogy: You might as well argue that "there are men" is a trivial analytic statement, since it follows from "John Smith is a man," which is analytic, since only a man could properly (that is, would conventionally) be named "John Smith." But although it is a contingent fact, not a logical necessity, that there are men, one could not possibly deny that there are men if one grasps the conventional meaning of *man*, for this word has acquired its meaning through ostensive definition—that is, through uttering the word *man* while pointing at a man. Similarly, the meaning of *attribute* and other type designations must be explained by examples before one can proceed to construct an abstract definition; hence, a disagreement about whether there are attributes is bound to be as verbal as would be a disagreement, if one ever broke out, about whether there are men. [A comprehensive and detailed analysis of the meanings of *analytic* and *necessary* is contained in A. Pap, *Semantics and Necessary Truth* (in press, Yale Univ. Press). For an acute discussion of the meanings of ontological statements, see M. G. White, *Towards Reunion in Philosophy* (Harvard Univ. Press, Cambridge, Mass., 1956)].
21. A. Fraenkel, *Einleitung in die Mengenlehre* (republished by Dover, New York, 1946), par. 14; R. L. Wilder, *Introduction to the Foundations of Mathematics* (Wiley, New York, 1952), chap. 10.
22. Indeed, finitists usually accept the natural numbers as, in some sense, "given"—in accordance with Kronecker's often cited statement that God himself created the natural numbers, though he left the construction of all the other kinds of numbers and classes to man. Sometimes it is the "rule of construction" of natural numbers (adding one) that is said to be intuitively given rather than the natural numbers themselves. But it is not clear in what sense a natural number can be said to be a mental construction. Acquiring an *idea* of a number n by "counting up" to it is, of course, a mental process, but when we ascribe to a class a particular number, we ascribe an attribute to it that cannot plausibly be identified with the mental process of counting.

ASSOCIATION AFFAIRS

Preliminary Announcement of Indianapolis Meeting and Call for Papers by AAAS Sections

The annual meeting of the American Association for the Advancement of Science for the year 1957, 26–31 Dec., will be the 124th national meeting. It will be the association's first meeting in the Midwest in 5 years and a return to Indianapolis, for the fourth time, after an interval of 20 years. Among those present will be a sizable number who can recall the successful AAAS meeting of 1937 and a whole generation of younger research scientists, teachers, and other science-minded professional men and women.

The fundamental, original purpose of the association when it was founded in September 1848, was ". . . by periodical and migratory meetings to promote [exchange of ideas among] those who are cultivating science." The annual meeting is still a major activity of the AAAS. With some 53,000 members and 271 affiliated scientific organizations, the association more than ever is prepared to consider matters of concern to all scientists and to society. The sessions of the 18 sections, which embrace all principal fields of science, and of the participating societies both meet the interests of specialists and provide unique opportunities for interdisciplinary programs. In recent years the association has also increasingly recognized its responsibility to inform the general public on developments in science and its obligation to assist in the encouragement of qualified young people to prepare for and enter careers in science, engineering, and education. The programs this year will reflect these objectives.

Center of the meeting will be the attractive and well-equipped Murat Temple which, with its capacious theater and three levels, will accommodate the larger sessions, the Annual Exposition of Science and Industry, the AAAS Main Registration-Information Center, AAAS Office, and the AAAS Science Theatre. The Temple will be the site of the AAAS Smoker for all registrants on the evening of 29 Dec. The Turners Athenaeum, immediately across the street, will provide additional session rooms. Although the Murat Temple and the Athenaeum are within walking distance of all downtown hotels there will also be special AAAS buses which will operate at frequent intervals between the Temple and the hotels. These will make it possible for everyone to visit the large-scale exhibits of the latest in the tools and materials scientists use and many of the technical accomplishments of large industries.

Among the downtown hotels, which have guaranteed a minimum of 2500 rooms for the AAAS, the remodeled Claypool will serve as AAAS headquarters. The Antlers has been assigned to the four science teaching societies, and the Marott to the American Astronomical Society. Other hotel headquarters assignments in the Sheraton-Lincoln, Continental, Severin, Warren, and Washington are announced in the advertising pages, along with housing reservation and advance registration coupons. As usual, advance registrants will receive the book-size General Program-Directory early in December by first class mail.

Local committees. No AAAS meeting could succeed without the interest and substantial assistance of local members and friends of the association. General chairman of the fourth Indianapolis Meeting is Adam H. Fiske, vice president, Eli Lilly and Company, and he has appointed the following as chairmen of their respective committees: Advisory, H. T. Pritchard, president, Indianapolis Power and Light Company; Exhibits, Edward B. Newill, president, Allison Division, General Motors Corporation; Finance, Joseph E. Cain, president, P. R. Mallory Company; Housing, William A. Atkins, president, Severin Hotel Company; Physical Arrangements, Newton G. Sprague, science and mathematics consultant, Indianapolis Public Schools; Public Relations, James W. Carr, Sr., executive secretary, Riley Memorial Association. An Honorary Reception Committee will also be formed.

Programs. Among the evening events will be the AAAS Presidential Address and Reception, 28 Dec. President Laurence H. Snyder will preside and Paul B. Sears will give his address as retiring president. The combined Sigma Xi-RESA address and the annual address of the United Chapters of Phi Beta Kappa are planned for 27 Dec., and the annual lecture and film of the National Geographic Society is scheduled for 29 Dec., preceding the AAAS Smoker.

The evening of 26 Dec. and the afternoon of 27 Dec. have been reserved for the two sessions of the AAAS General Symposium, "Moving frontiers of science II: Concepts that mold our lives," organized by the Committee on AAAS Meetings. Three eminent speakers, three panelists, a moderator, and a summarizer, all of whom will have met together well in advance, will discuss relativity,

organic evolution, and the individuality of man. Because of the general interest of this program, all AAAS sections and most of the participating societies have scheduled no sessions concurrent with it.

The 11th annual Junior Scientists Assembly, sponsored by the association's Academy Conference and cosponsored by the AAAS as a whole—a full afternoon program for high-school students interested in careers in science—will be arranged by a committee, of which Richard Armacost, department of biology, Purdue University, is chairman. The Academy Conference, composed of representatives of the 41 academies of science affiliated with the AAAS, this year will be concerned in large part with ways and means to assist the junior academies.

The sixth Conference on Scientific Editorial Problems will consider various aspects of communications and technical writing and editing. This year's program committee is headed by George L. Seielstad, Applied Physics Laboratory, Johns Hopkins, Silver Spring, Md. The recurrent Conference on Scientific Manpower, this year with two sessions described later, again will be arranged by a committee with Thomas J. Mills, National Science Foundation, as chairman. As the following synopsis of programs indicates, there will be interesting and important programs in all principal fields of science. The names given within parentheses are section officers or program chairmen.

Mathematics (A)

Section A (C. C. MacDuffee, University of Wisconsin) will sponsor an important symposium on the teaching of mathematics, to be arranged by John R. Mayor, director, AAAS Science Teaching Improvement Program. It will be cosponsored by the association as a whole and by the AAAS Cooperative Committee on the Teaching of Science and Mathematics. A session for invited and contributed papers on the mathematics of guided missiles is scheduled for 28 Dec. and the vice presidential address will be given by Mina Rees, Hunter College.

The recently affiliated *Association for Computing Machinery* (James E. Robertson, Digital Computer Laboratory, University of Illinois), will hold one of its two annual national meetings with the AAAS in Indianapolis.

Physics (B)

Section B (J. Howard McMillen, National Science Foundation) has scheduled a three-session symposium, "Spectroscopy," of 13 papers, arranged by K. W. Meissner, Purdue University, covering experimental, theoretical, and applied aspects of the subject. Part I, Friday morning, 27 Dec., will be devoted to wavelength standards, vacuum ultraviolet spectroscopy, and isotope shift. The second session on Saturday morning will deal with the spectra of lanthanides and actinides and with Lamb shift. Two papers to be given in the afternoon session of Saturday concern the theory of atomic spectra and the theory of spectra of diatomic molecules. Two other papers will be devoted to applications of spectroscopy. The speakers will be: V. A. Fassel and W. A. Gordon, Iowa State College; Mark Fred, Argonne National Laboratory; Gerhard Herzberg, National Research Council of Canada, Ottawa; Julian E. Mack, University of Wisconsin; J. Rand McNally, Jr., Oak Ridge National Laboratory; William F. Meggers, National Bureau of Standards; K. W. Meissner, Purdue University; R. S. Mulliken, University of Chicago; F. Rohrlich, State University of Iowa; Charlotte M. Sitterly, National Bureau of Standards; Frank S. Tomkins, Argonne National Laboratory; R. Tousey, U.S. Naval Research Laboratory; and P. G. Wilkinson, University of Chicago.

Special talks on neutrino physics, solid-state physics, and high-temperature plasma will also be given.

The physicists' dinner, 27 Dec., will be joint with *Sigma Pi Sigma* (James M. Kortwright, Purdue University) and will be followed by the vice-presidential address of Section B given by William F. Meggers, with Raymond T. Birge, University of California, presiding.

The *American Meteorological Society* (Kenneth Spengler, AMS, Boston) will cosponsor appropriate sessions.

Chemistry (C)

The program of *Section C* (F. E. Cislak, Reilly Tar & Chemical Corporation, Indianapolis) will consist of a two-session symposium on acetylene chemistry, another two-session symposium on pyridine chemistry, a series of sessions for contributed papers, and a vice presidential address by Cislak.

Alpha Chi Sigma (R. L. Hicks, Broad Ripple High School, Indianapolis) will have a dinner meeting and address for all chemists.

Astronomy (D)

The *American Astronomical Society* (J. Allen Hynek, Smithsonian Astrophysical Observatory, Cambridge, Mass.) has scheduled its 99th national meeting with the AAAS, 27–30 Dec. The J. I. Holcomb Observatory and Planetarium of Butler

University (Harry E. Crull, director) is cooperating. Features of the program will be the Helen Warner prize lecture, a symposium, "Distribution and evolution of intrinsically variable stars," and the annual dinner at which the vice presidential address of *Section D* will be given by Peter van de Kamp, Swarthmore College, with Chester B. Watts, U.S. Naval Observatory, presiding. With the AAS providing a full program of sessions for contributed papers and other events, Section D's program will be restricted to cosponsorship of the AAS program and the retiring vice presidential address.

The *Astronomical League* (Grace C. Scholz, Mason Hall Apartments, Alexandria, Va.) will have a half-day session.

Geology and Geography (E)

The program of *Section E* (Frank C. Whitmore, Jr., and Louis L. Ray, U.S. Geological Survey, Washington, D.C.) includes a symposium on the history of geology cosponsored by Section L; a symposium on karst phenomena, arranged by William E. Davies, U.S. Geological Survey, Washington, D.C., jointly with the National Speleological Society; a two-session symposium on the stratigraphy and structural geology of the Michigan and Illinois basins area; a four-session symposium, "Continental glaciation and its geographic importance as an environmental factor" with special emphasis on the midwestern states; and sessions for contributed papers in both geology and geography, these last jointly with the *Association of American Geographers* (Louis L. Ray, U.S. Geological Survey, Washington, D.C.), which will also cosponsor the glaciation symposium. As usual, the *Geological Society of America* will cosponsor all geologic sessions, including the Section E Smoker at which the retiring vice presidential address of Paul F. Kerr, Columbia University, will be given with Robert R. Shrock, MIT, presiding.

The *National Speleological Society* (Thomas C. Barr, Jr., 1100 Clifton Lane, Nashville, Tenn.), in addition to cosponsoring Section E's karst symposium, will have a session for papers and a symposium, "Caves and cave fauna of the Ohio River valley" on 28 Dec. The *National Geographic Society* will have its customary outstanding lecture and accompanying film on the evening of 29 Dec.

Zoological Sciences (F)

Section F (Harold H. Plough, Amherst College) will have sessions for contributed papers and demonstrations in several fields of zoology, cosponsored, it is anticipated, by appropriate societies; a symposium, "Recent advances in the understanding of pituitary function," arranged by W. R. Breneman, Indiana University; a two-session symposium, "Some unsolved problems in biology II," jointly with Section G; another symposium, "The geographic distribution of contemporary organisms"; a business meeting; and a dinner for all zoologists, jointly with the Society of Systematic Zoology, at which E. Raymond Hall, University of Kansas, will give the vice-presidential address of Section F. A symposium on radiation biology, sponsored by the Argonne National Laboratory (Austin M. Brues), is planned.

The national meeting of the *Society of Systematic Zoology* (R. E. Blackwelder, St. John Fisher College, Rochester, N.Y.) will follow the pattern of former meetings, with sessions for contributed papers and demonstrations, one or more panels or symposia, evening lectures, a tea, and a business meeting. A feature will be the annual SSZ Library and Lounge for all zoologists.

Biological Sciences (FG)

Beta Beta Beta Honorary Biological Society (Mrs. Frank G. Brooks, Box 336, Madison Square Station, New York 10, N.Y.) will hold its regular biennial meeting with the AAAS. The program—a business meeting, luncheon, and address—will be on 27 Dec. The *Ecological Society of America* (Stanley A. Cain, University of Michigan) will have its usual regional meeting with the association and plans three or more sessions for contributed papers and cosponsorship of appropriate symposia.

The annual national meeting of the *National Association of Biology Teachers* (Irene Hollenbeck, Southern Oregon College of Education) will begin with business meetings 26 Dec. It will include a separate session on "Outdoor biology" and a luncheon on 27 Dec., a joint meeting with the ANSS, NARST, and NSTA, and a session on "Methods and techniques: problem-solving in biology" on 28 Dec., with J. Darrell Barnard, New York University, the keynote speaker. A field trip with the American Nature Study Society, 29 Dec., will conclude their meeting. It is expected that the *American Society of Naturalists* (Bruce Wallace, Cold Spring Harbor, N.Y.) will either arrange a symposium or cosponsor one in the F, FG, or G series.

Botanical Sciences (G)

In addition to "Some unsolved problems in biology II," jointly with Section F, *Section G* (Barry Commoner, Washington University) will have sessions for contributed papers in the principal fields of botany, cosponsored probably by the *American Society of Plant Physiologists,* the

Ecological Society of America, and others. Other symposia are planned, and, following the annual botanists' dinner, the vice presidential address of Section G will be given by Harry J. Fuller, University of Illinois.

Anthropology (H)

The plans of *Section H* (Gabriel W. Lasker, School of Medicine, Wayne State University) are yet to be announced but probably will include one or more symposia and the vice-presidential address of William L. Straus, Jr., of Johns Hopkins University.

Psychology (I)

Section I (Clifford T. Morgan, Town Point, Cambridge, Md.) will have six sessions of invited papers covering the principal areas of research activity in psychology. The vice presidential address will be given by Neal Miller of Yale University.

The Society for Research in Child Development (Dale B. Harris, Institute of Child Welfare, University of Minnesota) is planning a regional meeting.

Social and Economic Sciences (K)

Section K and the *National Academy of Economics and Political Science* (Donald P. Ray, George Washington University) will jointly sponsor a symposium of general interest, "The social aspects of urban agglomeration." The three papers of this session, cosponsored by the *American Economic Association,* will deal with metropolitan expansion and public administration, the economic implications of urban growth, and statistical programming for problems of urban agglomeration—this last the vice-presidential address of Stuart A. Rice. *Pi Gamma Mu,* the national honorary society in the social sciences, it is anticipated, will have a dinner for the speakers. In addition, for the first time in many years, Section K will open sessions for contributed papers in the social and economic sciences.

The *American Sociological Society* (Raymond V. Bowers, Air Research and Development Command, and Conrad Taeuber, Bureau of the Census) will have a special meeting with the AAAS, and the *American Political Science Association* (Evron M. Kirkpatrick, APSA, Washington, D.C.) is considering a special program jointly with Section K.

The *Society for the Advancement of Criminology* (Donal E. J. MacNamara, New York Institute of Criminology) will have a four-session symposium, "Advances in scientific criminology," two luncheons with speakers, and an evening seminar on crime and police problems.

The *American Statistical Association* (Virgil L. Anderson, Statistical Laboratory, Purdue University) is planning a regional meeting, and the *Metric Association* (J. T. Johnson, Claremont, Calif.) may have a joint program with Section C or Np.

History and Philosophy of Science (L)

The program of *Section L* (C. Doris Hellman, c/o Pepper, 1065 Lexington Ave., New York 21, N.Y.) will include a symposium, "Man's changing view of his place in nature," a session for contributed papers, and a business meeting, all on 29 Dec. The *Philosophy of Science Association* (C. West Churchman, Case Institute of Technology) is planning several sessions, and the *Society for General Systems Research* (Richard L. Meier, 1137 E. Ann St., Ann Arbor, Mich.) will hold its annual meeting with the AAAS. One session will be "Organization—for humans, cells, and artifacts."

Engineering (M)

Section M will have a symposium of several sessions (Irving P. Orens, Newark College of Engineering) on "Measuring man's environment: the physical effects on man of light, temperature, sound, and air," cosponsored by the *American Industrial Hygiene Association* and others. The Engineering Manpower Commission will cosponsor the sessions of the Conference on Scientific Manpower.

Medical Sciences (N)

As in previous years, *Section N* will have a four-session symposium, the participants of which come from all parts of the continent. This symposium, cosponsored by the *Society for Investigative Dermatology* and the *American Medical Association Committee on Cosmetics,* is scheduled for 28 and 29 Dec. This year's subject, "The human integument: normal and abnormal" (Veronica L. Conley, AMA, Chicago) will include papers on the biochemistry and physiology of the normal skin, pathological conditions such as melanosis, and the therapeutic principles involved in correcting abnormalities; included will be the vice-presidential address of William B. Bean, and the announcements of the 13th Theobald Smith award, given by Eli Lilly and Company, the third AAAS-Anne Frankel Rosenthal Memorial Award in Cancer Research, and the second AAAS-Ida B. Gould Memorial Award for Research on Cardiovascular

Problems. Section N will cosponsor a variety of appropriate programs.

Alpha Epsilon Delta (Maurice L. Moore, national secretary) will have its customary morning session—this year on the coordination and integration of undergraduate and professional education in the health sciences—the annual luncheon with a speaker of prominence, and a tour of the facilities of the Indiana University Medical Center.

The *American Psychiatric Association* (Milton Greenblatt, Massachusetts Mental Health Center, Boston) will have a multisession symposium on the social aspects of tranquilizing drugs. Details of the special programs of the *American Association of Hospital Consultants* (Jacque B. Norman, Greenville, S.C.) and the *American Physiological Society* (Fred A. Hitchcock, Ohio State University) are not yet available.

Dentistry (Nd)

The two-day program of *Section Nd* (Maynard K. Hine and Joseph C. Muhler, School of Dentistry, Indiana University, Indianapolis), 27 and 28 Dec., will be centered at the Indiana University Medical Center. A three-session symposium, "The pharmacology and toxicology of fluorine," will deal with the effects of fluoride on the body excluding dental caries, with particular reference to cardiovascular diseases, kidney diseases, mortality and morbidity rates, effect on enzymes, effects of fluoride retention within the body, and a comparison between fluorides provided from natural and artificial sources. One or more sessions for contributed papers on fluoride will probably be opened, and group luncheons are planned. The entire program of the section will be cosponsored by the *American College of Dentists,* the *American Dental Association,* and the *International Association for Dental Research, North American Division.*

Pharmacy (Np)

The four-day program of *Section Np* (John E. Christian, School of Pharmacy, Purdue University) begins 27 Dec. with a morning session for contributed papers and continues with symposia, "The latest developments in the tranquilizer field —pharmacological, chemical, and clinical" and "Recent trends in new medication." Other sessions, for contributed papers, will include those of the *American Society of Hospital Pharmacists* (George E. Archambault, Pharmacy Branch, USPHS, Washington, D.C., in collaboration with Joseph Oddis, American Hospital Association, Chicago). There will be a pharmacists' dinner, a vice-presidential address by Robert C. Anderson, Eli Lilly and Company, and a social hour 28 Dec. After the session for contributed papers on 30 Dec., a half-day tour of the Eli Lilly laboratories is planned. Section Np's entire program will be cosponsored by the *American Association of Colleges of Pharmacy,* the *American College of Apothecaries,* the *American Pharmaceutical Association, Scientific Section,* and the American Society of Hospital Pharmacists.

Agriculture (O)

Section O (L. P. Reitz, Plant Industry Station, Beltsville, Md.) will have a four-session symposium on the biological and chemical control of plant and animal pests, 28–29 Dec., followed by two or more sessions for volunteer papers restricted to the same subject.

Industrial Science (P)

Section P (Frank C. Croxton, Battelle Memorial Institute) will have its second Industrial Citation Dinner on 26 Dec., a symposium of interest to leaders in industry the morning of 27 Dec., followed by a luncheon at which the retiring vice-presidential address of Monroe E. Spaght, Shell Oil Company, New York, will be given.

Education (Q)

The program of *Section Q* (Herbert A. Smith, University of Kansas) includes two sessions joint with the *International Council for Exceptional Children,* 26 Dec.; two sessions joint with the *American Educational Research Association,* 30 Dec.; some four sessions for contributed papers, 28–30 Dec.; a business meeting, 29 Dec., with the vice-presidential address of Arthur E. Traxler, Educational Records Bureau, New York, preceding it. The *AAAS Cooperative Committee on the Teaching of Science and Mathematics* (Bernard B. Watson, 2806 Blaine Drive, Chevy Chase, Md.) will cosponsor Section A's symposium on the teaching of mathematics.

The four science teaching societies meeting with the AAAS (coordinator, Paul Klinge, Thomas Carr Howe High School, Indianapolis) will have a joint mixer the evening of 27 Dec. The joint session the morning of 28 Dec. will complement the preceding AAAS General Symposium, "Concepts that mold our lives." The speakers and panel members will cover relativity, organic evolution, and the individuality of man with special reference to the implications for teachers and those whom they teach.

The separate portions of the program of the

regional meeting of the *National Science Teachers Association* (Richard W. Schulz, Emmerich Manual Training High School, Indianapolis, and Robert H. Carleton, NSTA, Washington, D.C.) consists of a meeting of the executive committee, a session and industrial field trips on 27 Dec., and three other sessions on 28 and 29 Dec. The *National Association for Research in Science Teaching* (George G. Mallinson, Western Michigan University) will have a two-session symposium, "Recent research in science education," on 27 Dec.

Science in General (X)

The annual *Academy Conference* (Father P. H. Yancey, Spring Hill College, Mobile, Ala.), composed of delegates and other members of the 41 academies affiliated with the association, will hold a day of sessions, 28 Dec., concluding with the annual dinner and presidential address by Mrs. B. G. Heatwole, Staunton, Va. Of particular concern this year will be a consideration of the proposals of the conference on junior academies held in Chicago, 15–16 Feb. The Academy Conference will sponsor the 11th annual Junior Scientists Assembly (Richard R. Armacost, Purdue University) the afternoon of 28 Dec.

The *American Geophysical Union* (Edward M. Brooks, Saint Louis University) is planning a regional meeting and will cosponsor appropriate programs.

The program of the annual national meeting of the *American Nature Study Society* (Richard L. Weaver, University of Michigan) begins with a board meeting 26 Dec. and includes, besides joint sessions with the science teaching societies, general sessions the morning of 27 Dec. and afternoon of 28 Dec. The annual dinner is 28 Dec., the annual showing of kodachromes, the evening of 29 Dec., following the joint field trip with the National Association of Biology Teachers.

The *Association of Technical Writers and Editors* (Richard Frehsee, International Business Machines Corporation, Endicott, N.Y.) may have sessions of its own in addition to participating in the program of the *Conference on Scientific Editorial Problems* mentioned earlier.

The recurrent *Conference on Scientific Manpower* (Thomas J. Mills, National Science Foundation) is planning two sessions of invited papers on "The status of scientists in a changing economy" and "Current scientific manpower developments at home and abroad," 30 Dec. As in previous years, this program will be cosponsored by the *Engineering Manpower Commission*, the *Scientific Manpower Commission*, the *National Academy of Sciences–National Research Council*, and the *National Science Foundation*.

The *National Foundation for Junior Museums* (John Ripley Forbes, NFJM, New York) is planning one or more sessions at the Indianapolis meeting on the work of the foundation.

The national meeting of *Sigma Delta Epsilon*, graduate women's science fraternity (Mrs. Helen Bolton Parker, Malvern, Pa.) begins with a business meeting on 26 Dec. and includes the annual dinner and grand chapter meeting on 27 Dec., and a luncheon for all women in science on 28 Dec. A headquarters room will be maintained throughout the meeting period.

The annual joint address of the *Society of the Sigma Xi* (Thomas T. Holme, Yale University) and the *Scientific Society of America* (Donald B. Prentice, Yale University) and award of the William Procter prize is scheduled for the evening of 27 Dec., following the 58th annual convention of Sigma Xi, the annual convention of RESA, and their joint luncheon on that day.

The 18th annual lecture of the *United Chapters of Phi Beta Kappa* (Carl Billman, Phi Beta Kappa, Washington, D.C.) is scheduled for the evening of 27 Dec.

Call for Papers by AAAS Sections

Eleven sections of the association will arrange sessions for contributed papers at the Indianapolis meeting. In three instances, as will be noted, Sections A, Nd, and O, the papers to be considered are restricted to the subjects stated. The secretaries or program chairmen to whom titles and abstracts should be sent, *not later than 30 Sept. 1957*, follow.

A–Mathematics. Mathematics of guided missiles. Dr. C. C. MacDuffee, Department of Mathematics, University of Wisconsin, Madison, Wis.

C–Chemistry. Dr. F. E. Cislak, Reilly Tar & Chemical Corp., 11 S. Meridian St., Indianapolis 4, Ind.

E–Geology and Geography. Geology: Dr. Frank C. Whitmore, Jr., U.S. Geological Survey, Washington 25, D.C. Geography: Dr. Louis L. Ray, U.S. Geological Survey, Washington 25, D.C.

F–Zoological Sciences. Dr. H. H. Plough, Biology Department, Amherst College, Amherst, Mass.

G–Botanical Sciences. Dr. Barry Commoner, Henry Shaw School of Botany, Washington University, St. Louis 5, Mo.

K–Social and Economic Sciences. Dr. Donald P.

Ray, Hall of Government, George Washington University, Washington 6, D.C.

L–History and Philosophy of Science. Dr. C. Doris Hellman, c/o Mrs. Morton Pepper, 1065 Lexington Ave., New York 21, N.Y.

Nd–Dentistry. Fluoride. Dr. Maynard K. Hine, School of Dentistry, Indiana University, 1121 W. Michigan St., Indianapolis 2, Ind.

Np–Pharmacy. Dr. John E. Christian, School of Pharmacy, Purdue University, Lafayette, Ind.

O–Agriculture. Biological and chemical control of plant and animal pests. Dr. Louis P. Reitz, Cereal Crops Section, Agricultural Research Service, USDA, Plant Industry Station, Beltsville, Md.

Q–Education. Dr. Herbert A. Smith, 205 Bailey, School of Education, University of Kansas, Lawrence, Kan.

Although the deadline is 30 Sept., most sections, and subsequently the AAAS office, would be glad to receive titles in advance of this date.

Raymond L. Taylor
Associate Administrative Secretary

AAAS Theobald Smith Award

The Theobald Smith award of $1000 and a bronze medal, which has been given yearly since 1937 (except for a lapse during the war years) by Eli Lilly and Company of Indianapolis, Ind., under the auspices of the AAAS, will be presented at the association's 124th meeting in Indianapolis, 26–31 Dec. Nominations are now being requested for the award. They may be made by fellows of the AAAS and should be sent to the secretary of the Section on Medical Sciences, Dr. Allan D. Bass, Department of Pharmacology, Vanderbilt University School of Medicine, Nashville 5, Tenn.

The prize is given for "demonstrated research in the field of the medical sciences, taking into consideration independence of thought and originality." Any investigator is eligible who was less than 35 years of age on 1 Jan. 1957 and who is a citizen of the United States. The research is not to be judged in comparison with the work of more mature and experienced investigators.

Nominations must be received *before 1 Sept.* The secretary requests that six copies of all data be submitted. The nomination should include a curriculum vitae, a statement summarizing the nominee's scientific contributions with an evaluation of their significance, and reprints of his or her more important publications. The vice president of Section N–Medical Sciences and four fellows will form the committee of award.

AAAS Dues Increase

At the meeting of the AAAS Council last December it was proposed from the floor that the association should consider raising its dues—a step which most other associations and scientific societies have found necessary within the past 10 years. This proposal was considered in detail by the AAAS Board of Directors at its March meeting.

Although there has been a gratifying increase in AAAS membership, which now stands at an all time high of approximately 53,000, there was a small deficit at the close of the Association's books for 1956. In the past few years, despite economies and continued efficient practices, the margin between income and outgo has been progressively narrower. Under these circumstances, the board concluded that the time had come for an increase in the annual dues of the association. Accordingly, the board voted unanimously to submit to the council a recommendation that the dues be increased from $6.50 to $8.50 per year, effective 1 January 1958. This recommendation met with approval by 87 percent of the large majority of the council members who returned the mail ballot.

The dues of the association were last changed, from $5 to $6.50, on 1 January 1948. They were changed then for the same reasons as now—a continued increase in such operating expenses as salaries and especially the printing costs of the association's journals and other publications. Then, as now, there was a reluctance on the part of the board and the council to curtail any AAAS activities which had proved their worth or to reduce the quality of the publications. As before, increases in membership and in advertising revenue have made it possible to postpone but not to avoid an increase in dues.

The AAAS has been fortunate in being able to charge smaller dues than most other associations. Two recent studies, one by the National Education Association and the other by the American Marketing Association, have shown that the median annual dues for professional associations are $10, which is also the median for associations with 20,000 or more members. AAAS dues have been in the low category for scientific and professional associations and can well remain below the median figure of $10 for the foreseeable future.

No one has to be told that the costs of goods have increased markedly in the last decade and that often the increase has occurred without a corresponding increase in the intrinsic value of the products. It is fortunate that the AAAS does not have to follow this pattern. The dues increase will be

enough to pay the increased costs of an enlarged weekly journal, the combined *Science* and *The Scientific Monthly,* as approved by the council last December. Thus, beginning next January, all members who now receive one of the journals will receive the enlarged weekly *Science,* and those who now receive both journals for $10 per year will get about the same amount of material for $8.50.

An equitable plan for putting the increase into effect has been developed. Present members whose terms expire on or before 1 October 1957 may renew their membership for an entire year at the current rate of $6.50. Similarly, new members who join on or before that date will be accepted at the same rate. Members whose terms begin at subsequent dates will be billed at the new rate of $8.50.

It is hoped that the combination of the two journals and the association's increased scope of activities will meet with approval by all present members and that a continued growth in association membership will occur during its 110th year. The AAAS has grown in its services to science and society and is needed today more than ever.

RAYMOND L. TAYLOR
Associate Administrative Secretary

BOOK REVIEWS

Franklin and His French Contemporaries. Alfred O. Aldridge. New York University Press, New York, 1957. 260 pp. $4.75.

Although Alfred Aldridge's new book is not concerned primarily with science, it will nevertheless be of interest to many scientists. The reason is that Benjamin Franklin's debut in France, the country of his greatest diplomatic triumphs, was as a scientist. Furthermore, France was the scene of the first trial of Franklin's most famous experiment, the test of whether clouds are electrified and the lightning discharge an electric phenomenon.

Franklin's *Experiments and Observations on Electricity, Made at Philadelphia in America,* one of the great books on science of the 18th century, was published in London in 1751 and soon came to the attention of Buffon, who sponsored a French translation in 1752. The French quickly appreciated the full measure of Franklin's achievement. Diderot, the philosopher and coeditor with d'Alembert of the *Encyclopedia,* wrote at once that in Franklin's book one could find the best possible exemplification of the way in which to interrogate nature by the experimental art. The experiments designed by Franklin showed the nature of electric discharges, the difference between pointed and nonpointed conductors, and the difference between grounded and nongrounded conductors and led to the analysis of the Leyden jar, or first condenser, and the electrification of storms. The new experiments attracted such great attention that they were performed for the King and Court, and soon the translator of Franklin's book brought to fruition the famous test of the electrification of clouds designed by Franklin but not performed by him. Franklin soon made an independent confirmation of the hypothesis of the electrification of clouds by means of the kite experiment, a second experiment which he designed and performed before the news of the French experiments reached him.

To gain some measure of Franklin's scientific reputation in France, it is necessary only to recall that the aforementioned translation went through two editions, then it was superseded by a brand new translation, printed in 1773. Several years before the American Revolution, Franklin was elected one of the eight foreign associates of the French Academy of Sciences, which meant that in that year it was generally agreed that he was considered to be among the eight greatest non-French scientists. To see what this event meant in the history of science, we have only to point out that the next American to be so designated was Louis Agassiz, but the work for which he was given this honor had been completed before he came to America.

Aldridge recounts in a stimulating and exciting way the introduction of Franklin's scientific ideas in France and the way in which the number of his admirers grew until he became an almost legendary figure. The success of his diplomatic mission, so crucial to America's winning her independence, is shown to be related to his initial renown as a scientist. One of the most interesting parts of Aldridge's book is the section showing how Franklin was portrayed in French fiction, poetry, and drama. Above all, Aldridge shows

clearly that the high place won by Franklin in French opinion was due in part to Franklin's own personal qualities and also to a recognition that greatness in science and sharpness of wit were admired without respect to nationality. All in all, Aldridge has given us an enlightening and entertaining picture.

I. BERNARD COHEN

Harvard University

Discovery of the Elements. Mary E. Weeks. Journal of Chemical Education, Easton, Pa., ed. 6, 1956. 910 pp. Illus. $10.

This is the revised, enlarged, and modernized sixth edition of a familiar and handy reference book. It provides information on the discovery of the chemical elements, the people who discovered them, and the situations under which they were discovered. It is fully illustrated with pictures of the discoverers, the tools with which they worked, and the places where they worked. Most of the illustrations were taken from the collection of F. B. Dains, professor of chemistry at the University of Kansas.

Henry M. Leicester, College of Physicians and Surgeons, San Francisco, California, edited this new edition and also contributed a chapter on "Elements discovered by atomic bombardment." Each chapter contains a large number of literature citations, which provide the reader with a ready and interesting guide to the original literature as well as documentation for the statements in the present volume.

A History of the Ancient Southwest. Harold S. Gladwin. Bond Wheelwright, Portland, Me., 1957. xx + 383 pp. Illus. $8.50.

Harold S. Gladwin's latest venture into the field of southwestern archeology has resulted in a large, handsomely designed, copiously illustrated, and well-printed book. It is also a controversial book, as Gladwin points out in his introduction. It will undoubtedly find much use in graduate seminars, in which students will be assigned segments to evaluate and criticize, for from his fertile imagination Gladwin has created much in the way of theory and data to criticize and evaluate.

In a review of this length one can neither detail all the motifs nor criticize them point for point. Nevertheless, a brief sampling may convey somewhat the character of the book. In Chapter III Gladwin sketches in summary fashion the peopling of North America and early man in the Southwest; in this summary he indulges in the pleasant pastime of battering down wide-open doors (page 23).

Chapters IV through XVII outline southwestern archeology—Gladwinian version. The Basket Makers are asserted to be Yuman Foragers. A southern Forager group, termed Cave Dwellers, is also described. A trait list (page 30) lists four traits possessed *only* by the Basket Makers. On page 35 Gladwin lists three of the four traits as also occurring among the Cave Dwellers—and, as a matter of fact, the fourth, unfired mud pottery, also occurs. On the other hand, several specific traits listed as shared (for example, bow and arrow, grooved maul) do not actually occur among the Basket Makers, and many others are so general that they lack meaning.

Pottery, architecture, 18-row corn, and a new physique, are introduced about A.D. 200 by an immigrant group, the Farmers, from the western plains. Pure Farmer sites are "Grand Gulch" and "Durango." Why the "Farmers" introduced only four whole vessels of reduced fired pottery into Grand Gulch (all known plains pottery is oxidized) and did not use it at their permanent village near Durango is not explained.

Later "Farmer" groups introduced pottery and architecture to the Basket Makers (Obelisk Cave), meanwhile inventing a new type of architecture, the crescent-shaped village as at site 13, Alkali Ridge, which is confined to the area north of the San Juan. Archeologists will be surprised to learn that Shabik'eshchee type of pottery decoration does not occur in the Durango area. They will be dumbfounded to learn that Basket Maker III, Pueblo I, and Pueblo II are contemporaneous regional variants (page 159).

The introduction of broad line red-on-orange pottery north of the San Juan is credited to a group of "Red" immigrants from eastern Mexico who began to civilize the Cochise Foragers just before A.D. 600. Rectangular design elements are introduced by the Red-Mogollones to the Farmer-Basket Makers about A.D. 700 (page 106), whereas the Red-Mogollon are credited with this development only between A.D. 800 and 900 (Chapter X). The Hohokam are introduced from western Mexico at A.D. 700.

From this sample it should be clear that Gladwin has selected data to fit his theories and ignored all else. Even in the data admitted there are many errors of fact as well as distortions of others. Most of the reconstructions put forth are based on dendrodates secured by Gladwin and wholly unchecked by anyone else. Radiocarbon dating is ignored, except that in passing it is mentioned as being an unreliable and untested method.

Perhaps the most cogent illustration of the selecting and ignoring of data is to be found in the bibliography. Only a single reference postdates 1949, and it is misquoted. Yet, during this 8-year interval more research has been published on southwestern problems than in any comparable period prior to 1949.

Despite the fact that this book fails to present an acceptable history of the ancient Southwest, it nevertheless offers a number of suggestions (many of which are not new) of influence and diffusion which should bear fruit in further work. The arguments arising from it will be long and loud—and that, perhaps, is what Gladwin wants.

JOE BEN WHEAT

University of Colorado Museum

Foundations of Inductive Logic. Roy Harrod. Harcourt, Brace, New York, 1957. 290 pp. $7.

The foundations of inductive logic continue to be a subject of lively interest and disagreement. In this ambitious book, a well-known economist unsparingly criticizes his distinguished forerunners and undertakes to renovate inductive logic "from the beginning onwards" (page 148). The author claims to have demonstrated that the principles of inductive logic are true a priori—are, as he puts it, "part of the logic of entailment" (page 50). Harrod's chosen principles are narrowly circumscribed in scope and correspondingly weak in application, but their claim to be a sufficient basis for all inductive inference is supported by lively and ingenious argument. Applications are made to a number of philosophic problems, including the existence of the external world and the epistemological status of memory.

If the author had demonstrated that "the essential principles of induction" are "co-equal in validity and authority and certainty with the principles of deduction" (preface, page vi), this would be the most important treatise on induction ever written. Such hopes will be disappointed by a careful examination of the argument.

The following considerations are advanced in favor of "the Principle of Experience," the indispensable foundation for all modes of inductive inference. Imagine yourself, says Harrod, "travelling along a continuity"—or, in less metaphorical language, noticing the persistence of some observable character, such as the constant pitch of a heard musical note, the visible presence of the moon in the sky, or the like. Now adopt the policy of *unceasingly predicting* that the "continuity" (that is, the persistence of the observed character) will last for *at least one-tenth* of the time beyond that for which it has already been continuously observed. (Thus, after 10 seconds' observation of the continuity, you predict it will last another second; after 20 seconds from the start, you predict a continuation for two more seconds; and so on.) Make such predictions incessantly until the "continuity" terminates, or until you choose to stop, whichever is the earlier. Then, *at worst*, you will be making wrong predictions only during the last one-eleventh of the journey. You are *sure* to be right most of the time!

The conclusion can also be stated in terms of probability. On Harrod's conception of probability, the probability of correctness of any *one* of the predictions of continuance is at least 10/11. If a smaller extrapolation ratio—say one one-hundredth—is chosen, the probability of successful prediction can be raised to at least 100/101. In general, the relevant probability can be brought as close to 1 as desired, by restricting the scope of the prediction accordingly. So, the chance of making a successful prediction can be made as high as we please.

Finally, if the policy is altered so that the "traveller" continually predicts a *constant* extension of the continuity—say a further duration of just 1 second rather than an extension in a fixed ratio—increasing duration of continuity will entail steady decrease of the extrapolation ratio, and so a steadily increasing probability of successful prediction. This is the "law of increasing probability of continuance" (page 63) or the "Principle of Experience."

Harrod says the principle "is known for certain to be true" (page 119). If he were right, we would know for certain that a particular inductive policy (the policy of proportional extrapolation, as we might call it) would necessarily be successful most of the time. By suitable choice of the proportion, we could guarantee success for 10/11, for 100/101, or, more generally, for $x/(x+1)$ of the time.

Now, all manner of principles can be known to work *hypothetically*: You can be sure of finding your way out of a maze by constantly preserving contact with one wall; you are bound to make money at roulette if you continue to double your losing stake. But there is no a priori guarantee that mazes will be found or that conditions for using infallible gambling systems will be allowed to exist. Harrod's policy of proportional extrapolation would certainly be successful if sufficiently long "continuities" were actually encountered in experience. But even the swiftest prediction takes time, and if the continuities were too brief, the policy would be inapplicable—as Harrod himself seems to concede (page 58).

How do we know that sufficiently long continuities will actually occur in the future? To this crucial question, Harrod replies: "All that nature contributes to the argument is a continuity. That is innocent enough in all conscience!" If begging the question is innocent, this is indeed innocent enough. Harrod has gone to great lengths to establish the truism that inductive inference will be successful if nature provides the conditions for the successful practice of inductive inference.

It is unnecessary to examine the fiction of continuous prediction that underlies Harrod's argument, or to consider the curious and, as I believe, fallacious arguments (pages 54–58) for obtaining a more refined probability estimate than the aforementioned $x/(x+1)$ figure. Harrod's ingenious theory of sampling is also vulnerable to criticism. But when the ship has a hole in the bottom, it hardly matters that the funnels are missing.

MAX BLACK

Cornell University

Discoveries and Opinions of Galileo. Translated with an introduction and notes by Stillman Drake. Doubleday, Garden City, N.Y., 1957. 302 pp. Illus. $1.25.

Four lively works, either previously untranslated or difficult to obtain in translation, are presented in *Discoveries and Opinions of Galileo* in slightly abridged form, with informative introductory sections, helpful footnotes, a bibliography, and an index. "The starry messenger" describes some of the things to be seen in the heavens through a telescope; "Letters on sunspots" speculates about the cause of these blemishes; "Letter to the Grand Duchess Christina" discusses the proper relationship of science to religion; and "The assayer" points out some errors in logic made by an influential Jesuit.

Integration. Edward J. McShane. Princeton University Press, Princeton, N.J., 1944. 394 pp. $2.95, paper.

Theory of Lie Groups. I. Claude Chevalley. Princeton University Press, Princeton, N.J., 1946. 213 pp. $2.75, paper.

These volumes, reissued by Princeton University Press in paperback edition, have been out of print for some time. They are numbers 7 and 8 in the *Princeton Mathematical Series*. The volumes have not been changed in any way from the original edition.

What Makes a College? A history of Bryn Mawr. Cornelia Meigs. Macmillan, New York, 1956. 277 pp. Illus. $5.

The bibliography of American college histories is not commonly among those most eagerly sought by impartial readers. Library stacks bearing rows of these institutional biographies tend, I fear, to become among the dustier and less often disturbed. The faithful chronicle, pursued through months of devoted toil by some retired dean or professor-on-leave, of how the Reverend So-and-So first conceived the idea of What's-Its-Name College, and how ensuing generations brought that college into full flower, becomes a handy Christmas memento from the college administrator to his trustees and other college friends, but it does not often rank as engrossing literature. The story tends to be much the same from college to college; the writing may be more earnest than inspired, the allusions and references are typically local, and too often the story has to be written guardedly because of the presence of founding families still in the nearby community.

Cornelia Meigs' *What Makes a College?*, a history of Bryn Mawr, is, as might be guessed from the foregoing paragraph, an exception. Allowing for an awkward phrase here and there, it is written with facility and clarity. It treats, with commendable candor, of the human conflicts which are part of the pattern in any college. Most importantly, the author shows keen appreciation, throughout the book, of fundamental issues in higher education which have both plagued and stimulated colleges from one end of the country to the other. *What Makes a College?* contains enough important educational problems to serve as a source book on several major issues: denominational restrictions versus cosmopolitanism; the place of graduate study in the undergraduate college; the place of quasivocational specialties, such as primary education, social work, and labor problems, in the liberal-arts setting; academic freedom and tenure—any of these could be expanded from this history of Bryn Mawr.

With all these important educational themes running through the story of Bryn Mawr's development, however, the theme which dominates is the place of women in higher education. To a certain extent this is in itself a historical theme, since very few educators today question the wisdom of giving women every possible educational opportunity. Yet the direction of that education toward fitting woman for her role in modern life is still far from a settled question. This is amply shown by widespread response to the Commission on the Educa-

tion of Women, recently formed under the auspices of the American Council on Education. The lively and well-deserved interest, among educators, in *What Makes a College?* might, in fact, be attributed to its lucid report of one institution's efforts to define and develop the best possible higher education for able young American women.

The towering figure throughout the book, both in terms of Bryn Mawr College and in terms of the theme of women's education, is M. Carey Thomas, president of Bryn Mawr from 1894 to 1922.

By the time Carey Thomas was elected to office, Bryn Mawr had had two leading spirits, both medical doctors and orthodox Quakers. The founder was Joseph Wright Taylor—brilliant but nonpracticing physician, notably successful yet curiously indifferent business investor, lifelong bachelor and deeply devout Friend, who in late years dedicated his wealth to the founding of what should be a model college for young women, struck, possibly, in the image of his beloved sister, Hannah. After Taylor's founding efforts had exhausted his somewhat delicate health, at the age of 70, a group of staunch Quakers around him carried on the plans for the new college. Its first president, James E. Rhoads, brought human wisdom and spiritual devotion to the infant college to the full extent that its founder could have wished.

Yet even before Bryn Mawr opened its doors (23 Sept. 1885) and from that time until she assumed the presidential office herself, it was Carey Thomas who put the fire and zeal into what was to become one of our most distinguished women's liberal arts colleges. When denied the position of first president, for which she had applied, she accepted second place as dean of the faculty and proceeded to install herself as Bryn Mawr's prime voice in all matters academic and in practically everything else. *What Makes a College?* is, to a large extent, the story of one gifted woman's struggle to lead America in recognition of maximum educational opportunity for women.

It is not for us to judge to what extent Carey Thomas' edifice at Bryn Mawr was her response to having been turned down, successively, by one American and two German universities before she was finally accepted for her doctor of philosophy degree, *summa cum laude,* at the University of Zurich. Certainly, some of her policies in governing the fortunes of Bryn Mawr for nearly four decades might cause the modern educator, man or woman, a few delicate shudders. She let nothing material or human come between herself and her aim to make intellectual rigor the sword, shield, and buckler of her college. (Woodrow Wilson, as a stern young teacher, figured in one of her early contretemps.) The ensuing years, under the unusually capable presidencies, successively, of Marion Park and Katharine McBride, have filled out and humanized Carey Thomas' precepts without destroying them. No longer does the graduating senior stand trembling, in cap and gown, literally on the carpet of the president's office, to be personally examined before she shall qualify for her degree; and no longer do whole departments go to sleep at night wondering whether they will be changed or possibly abolished, ere morning, by a stroke of the president's pen.

The story of these and many other developments at Bryn Mawr over its 70 years makes Cornelia Meigs' book fascinating reading, not only for the college administrator but for anyone with firsthand experience at a college which has set, as its star, the ideal of excellence. Throughout the history of Bryn Mawr College, intellectual integrity has shone forth as its chief characteristic. If the college has been blessed, also, with a wealthy and devoted clientele who turned up at strategic moments with major capital gifts, all credit belongs to Bryn Mawr for the way she has used her blessings. To read her history in the gradual development of a rich and varied college education of top quality, with all its concomitant problems, is to gain a fresh understanding of the meaning of liberal education for this day and the next.

LOUIS T. BENEZET

Colorado College

Learning and Instinct in Animals. W. H. Thorpe. Harvard University Press, Cambridge, Mass., 1956. 493 pp. Illus. $10.

Among the most significant of recent developments within the field of animal behavior has been the rise of the European school of thought that terms itself *ethology.* With its emphasis on the analysis of unlearned patterns of behavior and its principal concern with submammalian forms, ethology presents a contrast to American comparative psychology. Our own interests lie primarily with learning—with learning as we see it in the mammals and with learning theory, whose concepts have been gradually divorced from those of other disciplines. Although the two traditions have a common root, they have undergone a sharp divergence.

W. H. Thorpe's new book is a very commendable attempt to bring them back together. In it, the British ornithologist is trying to "point out to psychologists and learning theorists on the one hand and to zoologists and physiologists on the other

how dependent each is, or ought to be, on the work and viewpoint of the other." Much of it is given over to review; the part that is devoted to learning is the first completely contemporary treatise on the subject in the ethological literature. Many specialists will be infuriated by its frank eclecticism; others, doubtless, will welcome new support from areas where they never dreamed of looking. None except the frankest isolationist will find it unprovocative, and it has a flavor so distinctly Continental as to make it quite apparent that viewpoints which have fallen from domestic favor still are not without their champions.

Learning and Instinct in Animals presents an introduction to the concepts which, until now, have constituted ethology and includes, also, a systematic treatment of the recent work concerning unlearned behavior. It will find an audience, for the teachings of the school are only now beginning to be studied systematically by American experimentalists. The message will be lost to all but serious students of behavior, for this is clearly not a textbook. As a monograph, it serves a clear and present need.

DONALD R. MEYER

The Ohio State University

Man Unlimited. Technology's challenge to human endurance. Heinz Gartmann. Pantheon, New York, 1957. Illus. $4.50.

Man Unlimited is a relatively nontechnical, easily read account of human engineering and the psychophysiological aspects of modern living. In the foreword, Heinz Gartmann states: "Man himself is the weakest link in the chain of technology. . . . Each new machine and new instrument imposes new demands, new stresses. . . . After every new victory over resistant matter, man must now strive to cope with his own physical weakness. Again and again he is being forced to discover the limits of his capabilities and, if possible, to transcend them. With this in view, man has undertaken one of the most daring enterprises of our time: making the human being himself the subject of carefully planned stress tests." In the epilog Gartmann gives a fairly complete summary of the topics discussed in the book: "The mad dash to higher speeds, the cruel game of sudden deceleration, the plunge into the abyss of weightlessness, the broken rhythm of daily life, the perilous escape beyond man's natural living space, the fatigue of man subjected to innumerable stimuli, the possibility of devaluation of the human mind—these are external tokens of the great change."

Much of the detailed illustrative material is drawn from the annals of aviation medicine. He uses the descriptive adjectives and phrases characteristic of many German scientists' writing on technologic and scientific matters. The chapter titles are also indicative of the spectacular approach: "The mad dash," "The cruel game," "Into the abyss," "Life in a box," "The unending ordeal," and so forth. In spite of this manner of approach, the material is factual, and the illustrations and explanations of physiological and physical principles are sound.

Gartmann gives vivid and detailed descriptions of such stress tests as the deceleration tests conducted by John Paul Stapp; the human centrifuge tests at the Naval Air Development Center, Johnsville, Pennsylvania; the simulated bail-out experiments at Wright Field; the experiences of test and research pilots; the rocket flight experiments of Piccard; and the flights into the central stratosphere. In describing such tests he gives a brief but good account of such things as the composition and development of the earth's atmosphere and the physiological effects of pressure changes, oxygen concentrations, climatic changes, and noise.

Gartmann's last two chapters, in a sense, look into future problems of man—his "machines" and advanced technology. Here he describes the principles of atomic energy, the discovery of radiation, and the development of electronic computers and automatons.

This book is recommended for those who desire a brief but accurate, easily read, account of the ways in which man is coping with modern technology. It is particularly recommended for high-school science students. It will illustrate the utilization of chemistry, physics, biology, and mathematics in modern technology.

RAY G. DAGGS

American Physiological Society

Experiments with a Microscope. Nelson F. Beeler and Franklyn M. Branley. Crowell, New York, 1957. 154 pp. Illus. $2.75.

That all human institutions are mortal and that science is a human institution—these are premises that lead to a syllogism that we are, quite understandably, chary of completing. There are times, however, when even the most unwelcome of possibilities should be squarely faced. From the volume of current journalism devoted to the subject of recruiting scientists, we may reasonably infer that ours is such a time of candid self-appraisal. The national need for a continued influx of new

young people into science was never more clear, and statistical studies, though full of cross-currents as usual, suggest (at least) that the present growth of our cadres of scientists is not great enough for our needs. Biographical and other studies show that for science, as for other professions, it is important to "catch 'em while they're young." In solving the motivational problem, no age deserves more attention than the years of late elementary and early secondary education. We are all more than a little interested in what may be done to interest these young people in a scientific career.

The present slim book, apparently designed for the years between 10 and 15, is an altogether admirable introduction to the fascination of the microscope. The writing is simple but not dull. Step by step the authors lead the young student through a carefully graded series of trials and experiments. Studies with simple lenses (including convex droplets of water) precede work with a compound 'scope, and the theory of light and its behavior is simply presented. The young student is encouraged to experiment with different arrangements of light so as to know the possibilities of his instrument better. Adequate directions are given for studying blood and its circulation, protozoa, bacteria, molds, yeasts, and the structure of higher plants. There are directions for solving a variety of related problems of microscopy, using materials in the home or readily procurable; especially ingenious is the home-made microtome.

One proposal of the authors deserves comment because of its heterodoxy. In urging the young student to keep dated records (Chapter 7), the authors suggest that he keep them on 3- by 5-inch index cards. Such practice is distinctly contrary to the long-established tradition of using bound notebooks only, yet I think its proposal speaks well for the authors' understanding of their audience's psychology. With these youngsters, there is no serious question of legal priority or court battles. Diverse and fickle as their interests are (and should be), a bound notebook, possessing only one sort of order, and that the most trivial—the temporal order—would, in the end, discourage adequate note-making more than it would encourage it. Dated note-cards can always be rearranged, thus leading to discoveries that might be missed in a rigid notebook.

This delighted reviewer has only one suggestion for future editions: that the young microscopist be introduced to a simple method of measuring the things he sees. He can be told that his hair is about 80 microns in diameter (which will probably be not far from the truth), and by placing a hair in the field with the object of study he can get in the habit of estimating the size of the objects he observes. This is not a bad habit, and learning to write such data as "25μ" on his records will probably appeal to his sense of magic and the occult, a harmless enough motivation.

To all those who are fortunate enough to be able to influence the tastes of the coming generation, Beeler and Branley's little manual can be heartily recommended.

GARRETT HARDIN

Santa Barbara College

Scientific and Learned Societies of Great Britain. A handbook compiled from official sources. Allen and Unwin, London, ed. 58, 1956 (order from Macmillan, New York). 211 pp. $6.

More than 600 learned societies of Great Britain are listed, by subject, in this second postwar edition of *Scientific and Learned Societies of Great Britain.* There are 16 classifications, ranging from mathematics and physics to literature and fine arts. University societies are covered in a new section. As far as possible, the following information is given for each society: name and address, objectives, officers, number of members, subscription, meetings, publications, and library.

Part I, "Scientific research in Great Britain," covers the main research establishments administered by the Department of Scientific and Industrial Research, the Medical Research Council, and the Agricultural Research Council.

Indian Students on an American Campus. Richard D. Lambert and Marvin Bressler. University of Minnesota Press, Minneapolis, 1956. 122 pp. $3.

This little book is based on long interviews with a small number of students from India, Pakistan, and Ceylon about their lives as students at the University of Pennsylvania. Its aim is understanding rather than proof, coupled with an attempt to set up hypotheses about what happens in "cross-cultural education." The book succeeds in these matters with a minimum of jargon. Its main strength, however, lies in the presentation of the image of America and of India held by these students on entering and leaving a great impersonal American university. The book then utilizes an earlier study to follow somewhat similar students back to India to view their problems of readjustment to a nation where jobs are still scarce, where tangible signs of successful performance are very important, and where there is competition with

other ex-students who sport British school ties. The writers then cast back and make some sensible suggestions for improvements in the part American colleges can play in this total process.

The reader is able to get a clear, detailed picture of the typical day-to-day life of these students and to see the kinds of Americans they meet and how they try to fulfill their desires to become educated, to learn about America, and to be good ambassadors of their own countries. The small sample does not permit much analysis of the more personal motives and mechanisms involved, nor does it point up the peculiarly difficult problems that women from these areas often have in readjusting to their less free status on return home.

This volume is the first of a proposed series and is useful reading for any Americans who meet with students from Asia. It will provide some stimulating suggestions about how much "international understanding" we can reasonably expect from our exchange programs and just what the fate of such good will may be when it is buffeted by the winds of larger foreign policy actions. These students very rarely lose much of their sense of identity and reference as nationals. Any Americans who feel that such a tendency toward defection on the part of visiting students would be a good thing will be given moral and factual pause by this book.

WILLIAM W. LAMBERT

Center for Advanced Study in the Behavioral Sciences

Books Reviewed in SCIENCE

3 May

Prehistoric Settlement Patterns in the New World, G. R. Willey, Ed. (Wenner-Gren Foundation for Anthropological Research). Reviewed by F. H. H. Roberts, Jr.

Die Gattungen der Rhodophyceen, H. Kylin (Gleerup, Lund, Sweden). Reviewed by W. R. Taylor.

Physics and Chemistry of the Earth, vol. 1, L. H. Ahrens, K. Rankama, S. K. Runcorn, Eds. (McGraw-Hill). Reviewed by W. R. Thurston.

Chemistry of Chromium and Its Compounds, vol. 1 of *Chromium*, M. J. Udy, Ed. (Reinhold). Reviewed by J. T. Sterling.

10 May

Biochemical Individuality, R. J. Williams (Wiley). Reviewed by S. S. Kety.

Education in the U.S.A., W. K. Richmond (Philosophical Library). Reviewed by F. L. Wormald.

Advances in Genetics, vol. VIII, M. Demerec, Ed. (Academic Press). Reviewed by E. L. Green.

Principles of Fungicidal Action, vol. 30 of *New Series of Plant Science Books*, J. Horsfall (Chronica Botanica; Hafner). Reviewed by H. G. Shirk.

Principles of Guided Missile Design. Aerodynamics, Propulsion, Structures and Design Practice, E. A. Bonney, M. J. Zucrow, C. W. Besserer (Van Nostrand). Reviewed by R. Duffy.

17 May

The Reactive Intermediates of Organic Chemistry, J. E. Leffler (Interscience). Reviewed by C. H. DePuy.

Elasticity, Fracture and Flow, J. C. Jaeger (Methuen; Wiley). Reviewed by A. Kyrala.

Human Generation, Conclusions of Burdach, Dollinger and Von Baer, A. W. Meyer (Stanford Univ. Press; Geoffrey Cumberlege, Oxford Univ. Press). Reviewed by J. Needham.

Fleas, Flukes and Cuckoos, M. Rothschild and T. Clay (Macmillan). Reviewed by H. Friedmann.

Medical Sciences, ser. VII, *Progress in Nuclear Energy*, J. C. Bugher, J. Coursaget, J. F. Loutit, Eds. (McGraw-Hill). Reviewed by K. Z. Morgan.

24 May

William Heytesbury, C. Wilson (Univ. of Wisconsin Press). Reviewed by R. P. Multhauf.

Report of the Princeton Conference on the History of Philanthropy in the United States (Russell Sage Foundation). Reviewed by I. G. Wyllie.

Introductory Psychosomatic Dentistry, J. H. Manhold, Jr. (Appleton-Century-Crofts). Reviewed by J. Landa.

Annual Review of Nuclear Science, vol. 6, J. G. Beckerley, M. D. Kamen, L. I. Schiff (Annual Reviews). Reviewed by S. K. Allison.

Fusion Methods in Chemical Microscopy, W. C. McCrone, Jr. (Interscience). Reviewed by K. B. Krauskopf.

Physiologie de l'Insecte, R. Chauvin (Institut National de la Recherche Agronomique). Reviewed by J. Case.

Scientific Research in British Universities, 1955–56 (H. M. Stationery Office).

31 May

Franklin and Newton, I. B. Cohen (American Philosophical Society). Reviewed by M. Clagett.

The Biochemistry of Vitamin B_{12}, R. T. Williams, Ed. (Cambridge Univ. Press). Reviewed by B. F. Chow.

General Genetics, M. J. Sirks (Nijhoff, The Hague). Reviewed by G. W. Beadle.

Etude Radioanatomique de l'Os Temporal, M. Juster and H. Fischgold (Masson). Reviewed by M. W. Young.

Organic Analysis, vol. III, J. Mitchell, Jr., I. M. Kolthoff, E. S. Proskauer, A. Weissberger, Eds. (Interscience). Reviewed by L. Chalkley.

Psychological Research, B. J. Underwood (Appleton-Century-Crofts). Reviewed by C. G. Mueller.

Osteology of the Reptiles, A. S. Romer (Univ. of Chicago Press). Reviewed by E. C. Olson.

New Books

Vergleichende Physiologie. vol. III, *Ernahrung, Wasserhaushalt und Mineralhaushalt der Tiere.* W. von Buddenbrock. Birkhauser, Basel, Switzerland, 1956. 677 pp. DM. 66.

Handbuch der Physik. vol. XXX, *X-Rays.* S. Flügge, Ed. Springer, Berlin, 1957. 384 pp. DM. 88.

Magnetohydrodynamics. T. G. Cowling. Interscience, New York, 1957. 115 pp.

An Introduction to Electrostatic Precipitation in Theory and Practice. H. E. Rose and A. J. Wood. Essential Books, Fairlawn, N.J., 1956. 166 pp. $2.80.

The Growth of Leaves. Proceedings of the Third Easter School in Agricultural Science, University of Nottingham, 1956. F. L. Milthorpe. Butterworths, London, 1956. 233 pp. $6.80.

Truth and Meaning. David Greenwood. Philosophical Library, New York, 1957. 114 pp. $3.75.

Beginning Statistics. Lester Guest. Crowell, New York, 1957. 255 pp. $4.

Physics, a Modern Approach. L. Paul Elliott and William F. Wilcox. Macmillan, New York, 1957. 658 pp. $5.12.

The Fight for Fluoridation. Donald R. McNeil. Oxford University Press, New York, 1957. 241 pp. $5.

A Glossary of Mycology. Walter H. Snell and Esther A. Dick. Harvard University Press, Cambridge, 1957. 171 pp. $5.

Psychology You Can Use. Dorothy H. Yates. Crowell, New York, 1957. 248 pp. $3.95.

Chromosomes. Lectures held at the Conference on Chromosomes, Wageningen, 16–19 April 1956. Uitgevers-Maatschappij W. E. J. Tjeenk Willink, Zwolle, Netherlands, 1956. 231 pp.

Disaster A psychological essay. Martha Wolfenstein. Free Press, Glencoe, Ill., 1957. 231 pp. $4.

Modern Pulp and Paper Making. John B. Calkin, Ed. (previous editions by George S. Witham, Sr.). Reinhold, New York; Chapman & Hall, London, ed. 3, 1957. 549 pp. $10.

Sir Richard Gregory, His Life and Work. W. H. G. Armytage. Macmillan, London; St. Martin's Press, New York, 1957. 241 pp. $5.

Handbook of Snakes of the United States and Canada. vols. I and II. Albert H. Wright and Anna Allen Wright. Comstock Publishing Associates, Cornell University Press, Ithaca, N.Y., 1957. 1105 pp. $14.75.

Aspects of River Pollution. Louis Klein. Academic Press, New York; Butterworths, London, 1957. 621 pp. $14.50.

Boy's Book of Frogs, Toads, and Salamanders. Percy A. Morris. Ronald Press, New York, 1957. 240 pp. $4.

Ernest Rutherford, Atom Pioneer. John Howland. Philosophical Library, New York, 1957. 160 pp. $4.75.

Foundations of Radio. M. G. Scroggie. Iliffe, London; Philosophical Library, New York, 1957. 349 pp. $10.

The Horse-Flies (Diptera: Tabanidae) of the Ethiopian Region. vol. III, *Subfamilies: Chrysopinae, Scepsidinae and Pangoniinae and a revised classification.* H. Oldroyd. British Museum (Natural History), London, 1957. 489 pp. £4 10s.

Inventors and Inventions. C. D. Tuska. McGraw-Hill, New York, 1957. 174 pp. $3.75.

Psychological Tests and Personnel Decisions. Lee J. Cronbach and Goline C. Gleser. University of Illinois Press, Urbana, 1957. 165 pp. $3.50.

The Fundamentals of Electric Log Interpretation. M. R. J. Wyllie. Academic Press, New York, ed. 2, 1957. 176 pp. $4.50.

Expectant Motherhood. Nicholson J. Eastman. Little, Brown, Boston, ed. 3, 1957. 198 pp. $1.75.

Womanpower. A statement by the National Manpower Council with chapters by the council staff. National Manpower Council. Columbia University Press, New York, 1957. 371 pp. $5.

Working with Children in Science. Clark Hubler. Houghton Mifflin, Boston, 1957. 425 pp. $5.50.

America's Natural Resources. Edited for the Natural Resources Council of America by Charles H. Callison. Ronald Press, New York, 1957. 211 pp. $3.75.

The Doctor, His Patient and the Illness. Michael Balint. International Universities Press, New York, 1957. 355 pp. $7.50.

An Introduction to Astronomy. Robert H. Baker. Van Nostrand, Princeton, N.J., ed. 5, 1957. 333 pp. $4.85.

Automation in Business and Industry. Eugene M. Grabbe. Wiley, New York; Chapman & Hall, London, 1957. 611 pp. $10.

High-Speed Flight. E. Ower and J. L. Nayler. Philosophical Library, New York, 1957. 227 pp. $10.

The Neurohypophysis. H. Heller, Ed. Proceedings of the eighth symposium of the Colston Research Society held in the University of Bristol, 9–12 April 1956. H. Heller, Ed. Academic Press, New York: Butterworths, London, 1957. 275 pp. $9.50.

Encyclopedia of Instrumentation for Industrial Hygiene. Prepared and issued cooperatively by the University of Michigan Institute of Industrial Health and School of Public Health and the Occupational Health Program of the Public Health Service, U.S. Department of Health, Education, and Welfare. Charles D. Yaffe, Dohrman H. Byers, Andrew D. Hosey, Technical Editors. University of Michigan, Industrial Health Program, Ann Arbor, 1956. 1243 pp. $35.

From the Closed World to the Infinite Universe. Alexandre Koyre. Johns Hopkins Press, Baltimore, Md., 1957. 313 pp. $5.

A Textbook of Plant Virus Diseases. Kenneth M. Smith. Little, Brown, Boston, ed. 2, 1957. 652 pp. $12.

Thermodynamic Properties of the Elements. No. 18 of the Advances in Chemistry Series. Edited by the staff of *Industrial and Engineering Chemistry.* American Chemical Society, 1155 16 St., NW, Washington, D.C., 1957. 234 pp. $5.

Nuclear Reactor Physics. Raymond L. Murray. Prentice-Hall, Englewood Cliffs, N.J., 1957. 317 pp. $10.

1001 Questions Answered About the Seashore. N. J. Berrill and Jacquelyn Berrill. Dodd, Mead, New York, 1957. 317 pp. $5.

Vegetable Crops. Homer C. Thompson and William C. Kelly. McGraw-Hill, New York, rev. ed. 5, 1957. 619 pp. $8.50.

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Route-Mapping and Position-Locating in Unexplored Regions. Wilhelm Filchner, Erich Przybyllok, Toni Hagen. Academic Press, New York; Birkhauser, Basel, Switzerland, 1957. 288 pp. $9.

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Critical Notes on Evolution. William L. Fischer. William L. Fischer, Gabelsbergerstrasse 30, Muenchen, Germany, 1957. 111 pp.

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The Climate Near the Ground. Rudolf Geiger. Translated from ed. 2 German of *Das Klima der Bodennahen Luftschicht,* with revisions and enlargements by the author, by Milroy N. Stewart *et al.* Harvard University Press, Cambridge, Mass., rev. ed. 2, 1957. 514 pp. $6.

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Introduction to Organic Chemistry. Louis F. Fieser and Mary Fieser. Heath, Boston, 1957. 616 pp. $7.

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Statistical Methods in Research and Production. With special reference to the chemical industry. Owen L. Davies, Ed. Oliver and Boyd, Edinburgh, rev. ed. 3, 1957. 406 pp. 45s.

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Tranquilizing Drugs. A symposium held under the auspices of the American Association for the Advancement of Science in cooperation with the American Psychiatric Association and the American Physiological Society at the AAAS Atlanta, Ga., meeting, 27–28 Dec., 1955. Publ. No. 46. Harold E. Himwich, Ed. American Association for the Advancement of Science, Washington, D.C., 1957. 197 pp. $5; $4.50, AAAS members.

When Doctors Meet Reporters. A frank discussion by science writers and physicians of the controversy between the press and the medical profession. Compiled by Hillier Krieghbaum, from the record of a series of conferences sponsored by the Josiah Macy, Jr., Foundation. Published for the foundation by New York University Press, New York, 1957. 119 pp. $2.50.

Fundamentals of Sonar. J. Warren Horton. U.S. Naval Institute, Annapolis, Md., 1957. 387 pp. $10.

A Contribution to the Heritage of Every American. The conservation activities of John D. Rockefeller, Jr. Text by Nancy Newhall. Prolog by Fairfield Osborn. Epilog by Horace M. Albright. Knopf, New York, 1957. 179 pp.

Géologie Sedimentaire. Les series marines. Augustin Lombard. Masson, Paris; Vaillant-Carmanne, Liège, 1956. 722 pp. F. 11,000.

Proceedings of the Third International Congress on High-Speed Photography. Held under the auspices of the Department of Scientific and Industrial Research in London 10–15 Sept. 1956. R. B. Collins, Ed. Academic Press, New York; Butterworths, London, 1957. 417 pp. $13.

Practical Applications of Engineering Soil Maps. Engineering Soil Survey of New Jersey, Rept. No. 22. William W. Holman, Robert K. McCormack, James P. Minard, Alfred R. Jumikis. Rutgers University Press, New Brunswick, N.J., 1957. 113 pp. $3.

Cold Injury. Transactions of the fourth conference 7–9 Nov. 1955, Princeton, N.J. M. Irene Ferrer, Ed. Josiah Macy, Jr., Foundation, New York, 1956. 371 pp. $5.95.

Investigation of Virus Diseases of Brassica Crops. Agricultural Research Council Rept. Ser. No. 14. L. Broadbent. Cambridge University Press, New York, 1957. 94 pp. $3.

Nature's Guardians. Your Career in Conservation. Messner, New York, 1957. 192 pp. $3.50.

John Muir, Father of Our National Parks. Charles Norman. Messner, New York, 1957. 191 pp. $2.95.

Coal Science. Aspects of coal constitution. D. W. Van Krevelen and J. Schuyer. Elsevier, New York, 1957 (order from Van Nostrand, Princeton, N.J.) 352 pp. $9.50.

Meat Hygiene. WHO Monograph Series No. 33. World Health Organization, Geneva, 1957 (order from Columbia University Press, New York). 511 pp. $10.

Gestation. Transactions of the third conference 6–8 Mar. 1956, Princeton, N.J. Claude A. Villee, Ed. Josiah Macy, Jr., Foundation, New York, 1957. 253 pp. $4.75.

The Physiology of Reproducton in Fungi. Cambridge Monographs in Experimental Biology No. 6. Lilian E. Hawker. Cambridge University Press, New York, 1957. 128 pp. $3.

The Population of Jamaica. George W. Roberts. Published for the Conservation Foundation at Cambridge University Press, New York, 1957. 356 pp. $7.50.

Butterflies. E. B. Ford. Collins, London, ed. 3, 1957 (order from Macmillan, New York). 368 pp. $6.

Meetings

July

20–21. Medical-Sociological Aspects of Senile Nervous Diseases, internatl. symp., Venice, Italy. (S. N. Feingold, Jewish Vocational Service of Greater Boston, 70 Franklin St., Boston 10, Mass.)

21–28. Neurological Sciences, 1st internatl. cong., Brussels, Belgium. (P. Bailey, National Institutes of Health, Bethesda 14, Md.)

23–24. Modern Electrochemical Methods of Analysis, internatl. symp., Paris, France. (G. Charles, Ecole Superieure de Physique et de Chimie, 10, rue Vauquelin, Paris 5e.)

25–26. Structure Properties Relationships of Polymers (IUPAC), Paris, France. (International Union of Pure and Applied Chemistry, 4, Avenue de l'Observatoire, Paris 6e.)

25–29. Protein Chemistry Symp., IUPAC, Paris, France. (J. Roche, College de France, Place Marcellin Berthelot, Paris 5e.)

26–27. Experimental Psychology and Animal Behavior Section of Internatl. Union of Biology, Brussels, Belgium. (H. S. Langfeld, Dept. of Psychology, Princeton Univ., Princeton, N.J.)

26–27. Linguistic Soc. of America, Ann Arbor, Mich. (A. A. Hill, Box 7790, University Station, Austin 12, Tex.)

26–27. Military Psychology, internatl. symp., Brussels, Belgium. (National Academy of Sciences, 2101 Constitution Ave., NW, Washington 25.)

26–31. International Humanist and Ethical Union, 2nd cong., London, England. (American Humanist Assoc., Gate House, Yellow Springs, Ohio.)

26–1. International Congress on Nutrition, 4th, Paris, France. (4e Congrès International de Nutrition, CNERNA, 71, Boulevard Péreire, Paris 17e.)

27–3. Religion in the Age of Science, 4th annual, Star Island, Isles of Shoals, Portsmouth, N.H. (Mrs. R. Holt, Box 156, Pennington, N.J.)

28–1. Psychoanalysis, 20th internatl. cong., Paris, France. (Dr. Nacht, 187, rue Saint-Jacques, Paris 5e.)

28–3. Psychology, 15th internatl. cong., Brussels, Belgium. (L. Delys, 296, Avenue des Sept Bonniers, Forest-Bruxelles.)

31–5. International Assoc. for Hydraulic Research, Lisbon, Portugal. (M. C. Mendes da Rocha, Laboratorio Nacional de Engenharia Civil, Avenida do Brasil, Lisbon.)

31–6. Dermatology, 11th internatl. cong., Stockholm, Sweden. (C. H. Floden, Hudkliniken, Karolinska Sjukhuset, Stockholm 60.)

August

2–3. Pennsylvania Acad. of Science, Honesdale. (K. Dearolf, Public Museum and Art Gallery, Reading, Pa.)

5–11. Pan American Cong. of Pediatrics, 5th, Lima, Peru. (C. F. Krumdieck, Washington 914, Lima.)

5–17. Curare and Curare-Like Agents, internatl. symp., Rio de Janeiro, Brazil. (C. Chagas, Instituto de Biofisica, Universidade do Brasil, 458 Avenida Pasteur, Rio de Janeiro.)

6–9. Poultry Science Assoc., annual, Columbia, Mo. C. B. Ryan, Texas A. & M. College, College Station, Tex.)

7–9. Industrial Applications of X-Ray Analysis, 6th annual conf., Denver, Colo. (J. P. Blackledge, Metallurgy Div., Denver Research Inst., Univ. of Denver, Denver 10.)

7–9. International Union against the Venereal Diseases and the Treponematoses, 31st general assembly, Stockholm, Sweden. (Secretary General, Institut Alfred Fournier, 25, boulevard Saint-Jacques, Paris 14e, France.)

8–15. International Statistical Inst., 30th, Stockholm, Sweden. (Secretary General, ISI 30th session, Fack, Stockholm 5.)

8–15. International Union for the Scientific Study of Population, Stockholm, Sweden. (F. Lorimer, c/o American Univ., Washington 16.)

11–14. Heat Transfer, national conf., University Park, Pa. (G. M. Dusinberre, Pennsylvania State Univ., University Park.)

11–17. World Federation for Mental Health, 10th annual, Copenhagen, Denmark. (Miss E. M. Thornton, 19 Manchester St., London, W.1, England.)

12–16. Canadian Teachers' Federation, annual, Edmonton, Alberta, Canada. (G. C. Croskery, 444 MacLaren St., Ottawa 4, Ont.)

12–18. Theory of Functions, internatl. colloquium, Helsinki, Finland. (B. Eckmann, Ecole Polytechnique Federale, Zurich, Switzerland.)

12–25. International Soc. of Soil Mechanics and Foundation Engineering, 4th conf., London, England. (A. Banister, Institution of Civil Engineers, Great George St., London, S.W.1.)

18–21. American Astronomical Soc., Urbana, Ill. (J. A. Hynek, Smithsonian Astrophysical Observatory, 60 Garden St., Cambridge 38, Mass.)

19–21. National Council of Teachers of Mathematics, Northfield, Minn (M. H. Ahrendt, NCTM, 1201 16 St., NW, Washington 6.)

19–22. American Veterinary Medical Assoc., annual, Cleveland, Ohio. (J. G. Hardenbergh, AVMA, 600 S. Michigan Ave., Chicago 5, Ill.)

19–23. Clay Conf., 6th natl., Berkeley, Calif. (Dept. of Conferences and Special Activities, Univ. of California Extension, Berkeley 4.)

19–23. Clinical Chemistry, 2nd internatl. European Cong., Stockholm, Sweden (K. Agner, Box 12024, Stockholm 12.)

19–24. Finite Groups, International Colloquium, Tübingen, Germany. (H. Wielandt, Faculty of Mathematics and Natural Science, Eberhard-Karls-Universität, Tübingen.)

19–24. High Energy Physics Symp., Oak Ridge, Tenn. (University Relations Div., Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge.)

19–24. New England Assoc. of Chemistry Teachers, 19th summer conf., Waterville, Me. (Rev. J. A. Martus, College of the Holy Cross, Worcester 10, Mass.)

19–24. Origin of Life, internatl. symp., Moscow, U.S.S.R. (G. A. Deborin, Inst. of Biochemistry, U.S.S.R. Acad. of Sciences, B. Kaluzskaya 33, Moscow, B.71.)

20–22. Liquid Scintillation Counting Conf., Evanston, Ill. C. G. Bell, Jr., Technological Inst., Northwestern Univ., Evanston.)

20–23. Western Electronic Convention, annual, San Francisco, Calif. (D. B. Harris, Electron Tube Research, General Electric Microwave Lab., Palo Alto, Calif.)

21–24. Pi Lambda Theta, New York, N.Y. (Carmen Johnson, Pi Lambda Theta, 307 Portland Bldg., 1129 Vermont Ave., NW, Washington 5.)

22–5. International Scientific Radio Union, 12th general

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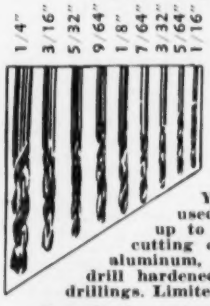

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assembly, Boulder, Colo. (K. A. Norton, Boulder Laboratories, National Bureau of Standards, Boulder.)

24–26. International Soc. for Biological Rhythm, 6th conf., Semmering, Austria. (A. Sollberger, Anatomical Dept., Karolinska Institutet, Stockholm 60, Sweden.)

25–27. Pacific Division–AAAS, annual, in conjunction with American Inst. of Biological Sciences, Stanford, Calif. (R. C. Miller, California Acad. of Sciences, Golden Gate Park, San Francisco, Calif.)

25–28. American Farm Economic Assoc., natl., Asheville, N.C. (L. S. Hardin, Dept. of Agricultural Economics, Purdue Univ., Lafayette, Ind.)

25–29. American Institute of Biological Sciences, annual, Stanford, Calif. (H. T. Cox, AIBS, 2000 P St., NW, Washington 6.)

The following 28 meetings are being held in conjunction with the AIBS meeting at Stanford, Calif.

American Bryological Soc., annual. (W. C. Steere, Dept. of Biological Sciences, Stanford Univ.)

American Fern Soc., annual. (I. L. Wiggins, Dept. of Biological Sciences, Stanford Univ.)

American Microscopical Soc., annual. (G. M. Smith, Dept. of Biological Sciences, Stanford Univ.)

American Phytopathological Soc., annual. (W. B. Hewitt, Dept. of Plant Pathology, Univ. of California, Davis.) 26–28 Aug. only.

American Soc. for Horticultural Science, annual. (H. P. Olmo, Dept. of Viticulture, Univ. of California, Davis.)

American Soc. of Human Genetics. (E. J. Gardner, Dept. of Zoology, Utah State College, Logan.)

American Soc. of Ichthyologists and Herpetologists. (R. C. Brown, Menlo College, Menlo Park, Calif.)

American Soc. of Limnology and Oceanography. (D. E. Wohlschlag, Dept. of Biological Sciences, Stanford Univ.)

American Soc. of Naturalists, annual. (D. Perkins, Dept. of Biological Sciences, Stanford Univ.)

American Soc. of Plant Physiologists, annual. (W. R. Briggs, Dept. of Biological Sciences, Stanford Univ.)

American Soc. of Plant Taxonomists, annual. (I. L. Wiggins, Dept. of Biological Sciences, Stanford Univ.)

American Soc. of Zoologists, annual (J. F. Oliphant, Dept. of Biological Sciences, Stanford Univ.)

Biometric Soc., WNAR. (D. E. Wohlschlag, Dept. of Biological Sciences, Stanford Univ.)

Botanical Soc. of America, annual. (W. C. Steere, Dept. of Biological Sciences, Stanford Univ.)

Ecological Soc. of America, annual. (I. L. Wiggins, Dept. of Biological Sciences, Stanford Univ.)

Genetics Soc. of America, annual. (D. Perkins, Dept. of Biological Sciences, Stanford Univ.)

Mycological Soc. of America, annual. (R. M. Page, Dept. of Biological Sciences, Stanford Univ.)

National Assoc. of Biology Teachers. (Miss E. Larson, Box 2841, Carmel, Calif.)

Nature Conservancy, annual. (W. Drake, Canyon, Calif.)

Phycological Soc. of America, annual. (G. F. Papenfuss, Dept of Botany, Univ. of California, Berkeley 4.)

Sigma Delta Epsilon. (Mrs. C. B. Parker, 7 Lloyd Rd., Malvern, Pa.)

Society for Experimental Biology and Medicine. (J. P. Baumberger, Dept. of Physiology, Stanford Univ.)

Society of General Physiologists. (D. Mazia, Univ. of California, Berkeley 4.)

Society for Industrial Microbiology, annual. (J. C. Lewis, Agricultural Research Service, USDA, 800 Buchanan St., Albany 10, Calif.)

Society for the Study of Evolution, annual. (R. W. Holm, Dept. of Biological Sciences, Stanford Univ.)

Society of Protozoologists, annual. (J. F. Oliphant, Dept. of Biological Sciences, Stanford Univ.)

Society of Systematic Zoology, Pacific section. (D P. Abbott, Hopkins Marine Station, Pacific Grove, Calif.)

Western Soc. of Naturalists, annual. (W. M. Hiesey, Carnegie Institution of Washington, Stanford, Calif.)

26–27. Mathematical Assoc. of America, 38th summer, University Park, Pa. (H. M. Gehman, Univ. of Buffalo, Buffalo 14, N.Y.)

26–28. Gas Dynamics Symp., 2nd, Evanston, Ill. (A. B. Cambel, Technological Inst., Northwestern Univ., Evanston.)

26–29. Boundary Layer Research, internatl. symp., Freiburg, Breisgau, Germany. (H. Görtler, Mathematisches Institut der Universität, Hebelstrasse 40, Freiburg, Breisgau.)

26–30. American Mathematical Soc., 62nd summer, University Park, Pa. (J. H. Curtiss, AMS, 190 Hope St., Providence 6, R.I.)

26–30. Infrared Spectroscopy Inst., 8th annual, Nashville, Tenn. (N. Fuson, Infrared Spectroscopy Inst., Fish Univ., Nashville 8.)

26–31. Low Temperature Physics and Chemistry, 5th internatl. conf., Madison, Wis. (J. R. Dillinger, Dept. of Physics, Univ. of Wisconsin, Madison 6.)

27. Society for Industrial and Applied Mathematics, summer, University Park, Pa. (D. L. Thomsen, Jr., 807 Enquirer Bldg., Cincinnati 2, Ohio.)

27–29. American Sociological Soc., annual, Washington, D.C. (Mrs. M. W. Riley, ASS, New York Univ., Washington Sq., New York 3.)

27–30. Biological Photographic Assoc., 27th annual, Rochester, Minn. (S. J. McComb, Section of Photography, Mayo Clinic, Rochester.)

28–30. Gas Chromatography, internatl. symp., East Lansing, Mich. (H. J. Noebels, IGC Symp., Instrument Soc. of America, 313 Sixth Ave., Pittsburgh, Pa.)

28–31. Soil Conservation Soc. of America, annual, Asilomar, Calif. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa.)

28–3. Cell Biology, 9th internatl. Cong., St. Andrews, Scotland. (H. G. Callan, Dept. of National History, Bell Pettigrew Museum, The University, St. Andrews, Fife.)

29–30. Computers and Data Processing, 4th annual symp., Denver, Colo. (J. M. Cavenah, Denver Research Inst., Univ. of Denver, Denver 10.)

29–30. Econometric Soc., European meeting, Luxemburg, Duchy of Luxemburg. (Econometric Soc., Box 1264, Yale Station, New Haven, Conn.)

29–31. Group Psychotherapy, 2nd internatl. cong., Zurich, Switzerland. (S. Lebovici, 3, Avenue President Wilson, Paris 16e, France.)

29–2. European Orthodontic Soc., annual, Genoa, Italy. (E. Fernex, 1, Place du Port, Genoa.)

29–3. International Geographical Cong., Tokyo, Japan. (W. W. Atwood, Jr., National Acad. of Sciences, 2101 Constitution Ave., Washington 25.)

30–5. American Psychological Assoc., annual, New York, N.Y. (R. W. Russell, APA, 1333 16 St., NW, Washington 6.)

31–2. Astronomical League, annual, Kansas City, Mo. (Mrs. W. A. Cherup, 4 Klopfer, Pittsburgh 9, Pa.)

31–3. Psychometric Soc., annual, New York, N.Y. (P. DuBois, Dept. of Psychology, Washington Univ., St. Louis, Mo.)

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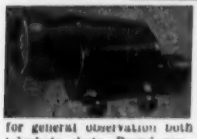

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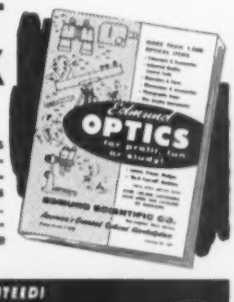

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